A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY 1799.

ARTICLE I.

Description of a new Instrument for drawing equidistant and other parallel Lines with great accuracy and expedition; intended principally for the use of Engravers. With Specimens of its Performance. By W. N.

SOME months ago I was informed by the Engraver who executes the plates for this Journal, that an ingenious artist had constructed a machine for ruling the shades and grounds of copper-plate engravings, which he not only used with great success in his own business, but had sold to others under an engagement of secrecy. How far this information might be strictly accurate, was of less consequence at the time this conversation took place, than the consideration that it would be easy to make such a tool, and render its advantages more general by publication. I then promised to make one; and having now finished it, I take the earliest opportunity of communicating it to the world. From the preceding historical statement, the reader will see that I have no claim to the original thought of substituting mechanical operation instead of hand work in this department of engraving; and those who have seen the screw gear of Ramsden's great dividing engine will also perceive, that I have done little more than distribute the parts of this tool in what appeared to me to be the most simple and convenient manner.

Fig. 1. Plate XX. represents the instrument of the size of that which I have made. The outline section, fig. 2, represents the same viewed in a direction parallel to the edge of the moveable ruler. The letters denote the same things in both. A A A A is a frame fixed to the drawing-board. It resembles that of a sliding rule, and serves to guide a sliding piece, which, lying in fig. 1, immediately under the screw, could not with convenience be denoted by any letter. B C is a screw of exactly forty threads in the inch, but might conveniently be made of a coarser thread. G H and E D are two cocks, the former of which, G H, is fixed to the frame A, and bears a clip or pair of nuts, which open and shut with a joint like a pair of compasses, and either embrace the screw by a re-

gularly tapped part when ſhut, or leave it at liberty when open. Fig. 3. ſhews the face of this cock with the clip, of which P is the joint, Q Q tapped part, R a pin to inſure their coming up fairly, and S a claw, which, when drawn upwards, ſerves to cloſe them; or, if thrown back, leaves them at liberty to open by the action of a ſmall ſpiral ſpring, let into oppoſite holes near the joint. E D, fig. 1, repreſents the other cock, which is fixed to the ſliding piece. It carries the ſteel ruler E F, which, though ſufficiently ſtrong, is thin enough to adapt itſelf to ſlight variations of thickneſs of the plate beneath it. In my inſtrument I have made it adjuſtable to much greater variations of thickneſs, by means of an horizontal axis; but as this contrivance adds to the expence, and diminiſhes the ſimplicity of the inſtrument, I would rather recommend that great variations ſhould be allowed for by putting paper or thin ſlips of metal underneath the plate A A, as may be required. The end C of the ſcrew is turned down, and fixed in the cock E D by means of a nut and waſher; or in any other of the methods which are familiar to inſtrument-makers. The upper part of the cock E D is filed round, and cut into teeth, of which fifty would complete the whole circle. The centre of this external circular part correſponds with the axis of the ſcrew (ſee fig. 4). L, M repreſent two ſhort cylindrical pieces which are hollow, and apply to each other ſo as to form a kind of box. Within, and fixed to the part M, which is fixed to the ſcrew itſelf, there is a ratchet wheel divided into fifty teeth; and within the part L, which is merely ſupported by the cylindrical part or ſtem of the ſcrew, there is a ratchet, which holds when the part L is moved by its handle N from right to left, but eſcapes when that handle is moved in the contrary direction. O is a lever, or arm, likewiſe ſupported by the ſtem of the ſcrew, and occupying the remaining ſpace between the handle N and the cock D. At the outer extremity of this lever there is a ſmall ſteel blade, which, by means of a back ſpring exactly reſembling that of a pocket knife, may be made to form a continuation of the lever itſelf, or, by being placed at right angles to the lever, may be made to reſt in any of the diviſions between the teeth of the circumference of the cock; and conſequently will, by that means, confine the lever to the poſition in which it is placed. The handle N cannot paſs the lever O, becauſe this laſt is too thick, and there is a ſtud or pin T, fig. 4, upon the face of the cock D, which prevents the handle from being moved beyond a certain determinate ſtation to the left hand. And, laſtly, the ruler I K, which is repreſented as broken off at K, but (like E F) may be of any required length, ſerves, by means of a thumb ſcrew at I, and another at the oppoſite end, to ſecure the copper-plate againſt the drawing-board in the uſual manner.

After this deſcription the uſe of the apparatus may be eaſily underſtood. By drawing back the claw S, fig. 3. the ſcrew is ſet at liberty, and the ruler E F may be brought to any required diſtance from I K by hand. The plate may then be duly placed and ſecured to the board, and the clip drawn gently together by the claw S. In this ſituation, ſuppoſe the lever O to be placed at a conſiderable diſtance from the handle N, that handle may be moved to the right, during which the click will gather upon the ratchet wheel; and then being returned to the left, it will carry the ſcrew round. The gentle preſſure, exerted by means of the claw, will tend to cloſe the clip upon the ſcrew, as ſoon as it comes into a fair poſition by its rotation; at which inſtant the claw will ſuddenly fall into its place, and the machine is ready for work, excepting that the adjuſtment for the fineneſs or coarſeneſs of the

the ſtroke muſt firſt be made. This is done by the lever O. If the ſteel blade be dropped into the firſt notch beginning on the left hand, the handle N will be confined; if at the ſecond notch, the handle, upon being moved backwards and forwards between the pin T and the lever O, will move the ſcrew through one tooth, or one-fiftieth of a turn, each time, and conſequently will carry the ruler F through one 2000th part of an inch. If the blade of the lever O be placed in any other of the notches (which are numbered by filing the corners of every fifth notch), the quantity paſſed over, at each return of the handle, will be greater or leſs according to the number. As there are but twenty-ſix notches, the greateſt ſingle ſhift of this inſtrument will be one-eightieth part of an inch; but as the ſhift is ſo readily made, it is eaſy, even with this fine ſcrew, to reach greater intervals, by moving the handle once, twice, or even three times, between ſtroke and ſtroke. Thus for one-fiftieth of an inch, or $\frac{40}{2000}$ths, the number of intervals cannot be paſſed over at one ſtroke; but, if the blade be ſet at the twentieth notch, the ruler will be ſhifted exactly that quantity by two movements of the handle. It is needleſs to multiply directions for this part of the operation; and the more ſo, as the artiſt is much more likely to determine the fineneſs of his ſhade by previous trials, which will render him acquainted with the inſtrument, than by any numerical computation.

An inſtrument like the preſent may alſo be uſefully employed in ſubdividing lines into ſmall parts. If curved lines be required to be drawn at equal diſtances from each other, the ſimpleſt method appears to be that of varying the figure of a temporary edge of horn or metal, which may be fixed to E F: and if lines be required converging to a point, the ruler E F may be made to occupy the radius of a circle of any magnitude whatever, by giving an adjuſtable angular figure to the ſlider, and cauſing it to move againſt two points, upon principles deduced from the thirty-firſt propoſition of Euclid's third book.

As this inſtrument was made at intervals of leiſure, without any account being kept of time or materials, I cannot diſtinctly ſtate the coſt: but I eſtimate that the whole being made of braſs and ſteel, and well finiſhed, with a ſmall drawing-board and the wooden holding piece I K, might be afforded by a ſhopkeeper in the mathematical-inſtrument buſineſs for ten guineas. If the frame and ſlider were of box-wood, which I have no doubt would anſwer very well, and the ornamental covering L M of the ratchet-wheel were omitted, I ſuppoſe the inſtrument could be afforded for conſiderably leſs than half that ſum.

The engraver has uſed this apparatus in the plates of the preſent number.

II.

Memoir on the Climate of Ireland. By the Rev. WILLIAM HAMILTON, *of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M. R. I. A. Correſponding Member of the Royal Society of Edinburgh, &c.*

[Concluded from Page 386, Vol. II.]

Of the Effects of theſe Weſtern Tempeſts on the General Temperature of the Climate.

IN a paper, honoured with a place in the ſecond volume of the Tranſactions of the Academy, I recorded the reſult of ſome experiments made in the year 1788, for the purpoſe of aſcertaining the temperature of the earth in our latitudes.

From these experiments it appears, that the medium temperature of the kingdom of Ireland, from its southern to its northern extremity, nearly coincides with the standard assigned to corresponding degrees of latitude in Mr. Kirwan's ingenious tables *.

It is further discoverable, that the general warmth of our island is in no respect inferior to that of other countries in its parallel: from whence we are naturally led to conclude, that the annual quantity of heat received here is not less in our day than it was in former ages.

If these things are so, it may be denied that there is any substantial foundation for the frequent complaint of change in our seasons. The ocean may be agitated by storms, sands be drifted along our coasts, and trees blasted in mountainous and exposed situations; but why should the farmer lament the diminished ardour and shortened period of his summers, as if winter possessed more than its natural portion of the revolving year? Whence should arise the gardener's complaints, that his peaches, however sheltered from storms, rarely now arrive at maturity; and that his May-duke cherries, even in the most favoured aspects, no longer merit that distinguished title?

May I be permitted to assume, as true, the fact which it has hitherto been the purpose of this paper to demonstrate, and in support of which the trees, the sands, and the tides of our island seem to bear constant and unequivocal testimony; namely that, of late years, the western winds have blown with increased violence? From that single principle an answer may be deduced to all these interesting questions.

It is a matter well known, and easily to be explained, that the surface of the ocean varies less from the mean annual temperature of its latitude, than land in the same parallel †: that is, the surface of the sea is colder in summer, and warmer in winter, than the surface of the ground in that latitude.

If then the prevalent winds of any country blow over an ocean situated in its parallel, that country will relatively be denominated temperate; it will be free from all extremes; the heats of summer and the colds of winter will be checked by sea breezes of a contrary property; and the land, influenced by the neighbouring element, must, more or less, partake in its equability of temperature.

Such is the case in almost all the islands of the world: and such from immemorial ages has been the peculiar character of Ireland. Hither come the western winds, modified by the temperature of the broad Atlantic ocean, which they traverse in their career: hither fly the clouds, teeming with moisture collected in a course of three thousand miles along its surface; and here uniformity of temperature, and redundant humidity, have always been marked as the distinguished character of our soil.

Since then the trees, the sands, and the tides demonstrate that these winds have, of late years, blown with unusual violence; since they bear testimony that a large quantity of air thus directed, thus tempered, and surcharged, has passed over our lands; it plainly follows that the climate must have felt the change; that it must have experienced colder summers and milder winters than heretofore, approaching towards that equability of heat and redundance of moisture which the farmer and the gardener at present so heavily lament.

But it is not from increase of quantity alone that these winds have produced their effects.

* See Kirwan's Estimate of Temperatures. † Ibid.

They

They have altered the temperature of the ocean itſelf; and thus have, as it were, multiplied their changeful influence on the land; acting there at once with new properties, as well as with increaſed quantity.

The ſurface of water in a ſtate of tranquillity admits of greater variations of temperature than in a ſtate of agitation. It may become much hotter in ſummer and colder in winter when calm than when diſturbed; for the particles at the ſurface, when heated or cooled, do not immediately give place to others nearer the bottom; the proceſs of commixture, in a tranquil ſtate, is gradual, and the tranſmiſſion of change ſomewhat reſembles the ſlow and retarded progreſs that takes place on the land. Agitation always counteracts this gradual proceſs; a rapid commixture of the particles produces a quick aſſimilation of temperature throughout the whole maſs, and, thus taking away all partial exceſs, reduces the whole toward a medium ſtate of uniformity.

Thus, ſummer tempeſts always tend to diminiſh the ſuperficial heat of an ocean over which they ruſh; while winter blaſts, agitating the waters at conſiderable depths, reſiſt the natural cold of the ſeaſon by a ſupply of relatively warm particles, which ariſe from the bottom toward the ſurface.

That the Atlantic ocean, the vaſt and potent arbiter of our ſeaſons, has ſuffered unuſual agitation of late years, ſeems evident from the natural phenomena recorded in the beginning of this Memoir. The trees and ſands of our iſland bear teſtimony to the ſtorms which ſweep along its ſurface; and the tides come to us as frequent and unerring meſſengers of the tumultuous and agitated ſtate of its billows attendant on their furious career.

From this perturbed abyſs of waters has ariſen an unceaſing influence, equally potent to check the ardour of the ſummer ſolar beams, or to relax the ſhackles of a northern winter. By this prevailing influence, ſummer has been rendered impotent to raiſe and ripen many of our crops; and the farmer, taught by neceſſity, learns now to hand them over ready grown, and prepared for the maturity of ſummer under the mild temperature of an Atlantic winter. Hence too the gardener has, of late years, been compelled to call in the aid of artificial heat to forward the peach, the nectarine, the grape, and every other ſpecies of delicate fruit to perfection.

Hence premiums now vainly attempt the growth of hemp, which formerly peaſants, in the moſt northern counties of Ireland, cultivated without reward around their cabins for domeſtic uſe. And hence the ancient apiaries of our iſland, once ſo celebrated, and guarded with ſuch ſpecial and minute attention by the Brehon laws *, are now extinct; and honey, from being a common article of popular conſumption, has become a rare luxury, or an expenſive medicine.

Winter has likewiſe felt the general influence of this Atlantic temperature; our graſſes ſcarcely droop beneath the froſts; wheat and oats vegetate in the open fields during the very ſolſtice itſelf; myrtles and laurels, in ſheltered ſituations, brave the ſeverity of winter; the Foyle, and other large rivers of the northern province, frequently ſubject to the icy chains of former ages, now run in uninterrupted freedom †.

* See Collectanea de Reb. Hib. Tranſl. Brehon Laws.

† Fifty years have elapſed ſince the river Foyle has been effectually frozen over at Derry. It is alſo obſerved that the river Thames, in Britain, is leſs frequently frozen of late years than formerly. See Archæologia Britannica, vol. iii. p. 55.

Facts,

Facts such as these are to be considered as the basis of general opinion concerning the alteration of our climate; while old age, connecting these appearances with the fragility of declining life, and a decayed constitution, has become querulous in proportion to its feelings, and, judging of external phenomena by the exaggerated test of its own acute sensations, emphatically pronounces that the seasons are now less favourable than formerly.

On the Consequences of this Equability of Temperature to Animal and Vegetable Life.

FROM the transient perusal of this Memoir, a rapid and impatient mind would probably draw numberless false conclusions. The facts here recorded seem to wear a gloomy aspect; to mark a gradual deterioration of our seasons; to indicate a climate, harsh, ungenial, and of consequence sterile in its nature; clouded, humid, tempestuous, cheerless, and unfriendly to animal and vegetable life.

All these conclusions contradict experience, and may be overturned by a calm consideration of the phenomena themselves.

Experience teaches us that dry seasons and easterly gales are, in our island, invariable sources of feeble vegetation and numerous diseases*; and the history of the world informs us that winds, whether hot or cold, are in their nature deleterious to animal and vegetable life, in proportion as they become deprived of humidity. The warm air of the African desert breathes desolation over the parched land of Egypt and Syria, before it is yet felt as a tempest †. Even our own westerly winds, the Atlantic messengers of health and fertility to our island, after traversing the cold and dry tract of the vast Siberian continent, bear nothing but sterility, and almost perpetual winter, to the unhappy climate of Kamschatka ‡.

Heat or cold in extremes, dry air in rapid motion, and moist air in a stagnant state, seem to be the principal external sources of human diseases; and climates are generally found favourable to health and longevity in proportion as they are exempt from these natural causes of disorder and decay.

It ought therefore to be inferred, *a priori*, that Ireland, celebrated for the singular equability of its temperature, and the ceaseless motion of an atmosphere always influenced by the moisture of the Atlantic, should be likewise characteristically free from natural disease; and experience proves that this conclusion is true.

The exhausting agues of North America, or the fens of England §; the fatal fluxes which prevail in the low countries of the continent of Europe; the dreadful bilious distempers of both the Indies; the pestilence which desolates the African and Asiatic climates,

* Of this the influenza, attendant on the easterly winds of spring in the present year (1795), affords a strong instance.

† See Volney's account of Egypt and Syria.

‡ The latitude of Kamschatka corresponds with that of Ireland; the westerly winds are prevalent in each; yet the former experiences a rigorous winter of nine months, and the latter rarely of as many days. See Cook's Voyage in 1779, vol. iii. ch. 6.

§ The ague is so rare in several parts of Ireland, that many persons are totally unacquainted with it. In the northern province the author has never met with the disease, and its existence there is generally denied.

mates, are all either entirely unknown, or but feebly felt in Ireland. There is here no characteriſtic diſeaſe to mark the natural ſource of unhealthfulneſs. There are few diſorders which cannot be directly traced up to ſome artificial cauſe; to ſome intemperance, to ſome neglect, to ſome exceſs, either of luxury or penury, in the ſuffering ſubject. Manufactures in their kind unwholeſome, induſtry exerted beyond its proper limits, irregularity of food or fermented liquors, illicit amours, colds, the conſequence of folly or inattention, exceſs or deficiency of proper exerciſe, anxiety and fretfulneſs of mind, together with the copious liſt of caſualties, afford to the phyſician of this country the cauſes and explanation of almoſt all its diſorders.

From its peculiar ſalubrity, the natives of this iſland are celebrated through Europe for juſt ſymmetry of proportion and an athletic frame; becauſe, from earlieſt infancy to manhood, a check is rarely given to the progreſſive increaſe of animal ſtrength, or the natural and appropriate forms of an undiſeaſed body. From the ſame hygeian ſource flow thoſe ardent paſſions, thoſe exuberant ſtreams of animal ſpirits, which render our natives always cheerful, oftentimes turbulent and boiſterous, the uſual conſequences of uninterrupted health and a vigorous conſtitution. Hence wild adventure, perſonal courage, impetuoſity of purſuit, inattention to conſequences, and improvidence of diſpoſition, become the characteriſtic features of minds ſeldom under the influence of that anxiety, which, flying from the preſent moment and its enjoyments, watches for futurity, and pants after remote felicity.

The general temperature of our climate, in the vicinity of the capital, is ſomewhat lower than the 50th degree of Fahrenheit's thermometer*; and a mean of the hotteſt or coldeſt months of our year rarely varies more than ten degrees from this ſtandard heat †. Winter therefore with us is uſually accompanied with a temperature of forty degrees, ſpring and autumn of fifty degrees, the ſummer of ſixty degrees of the thermometer: and the general heat of any ſingle month of theſe ſeveral ſeaſons ſeldom varies much from the correſponding temperature of that particular ſeaſon to which it belongs.

Of theſe limits the loweſt is not ſufficiently cold to check the growth of any of the natural herbage of our iſland, nor the higheſt powerful enough to parch the ſurface of a moiſt ſoil, or to ſcorch its luxuriant graſſes. Hence it comes to paſs that our fields maintain a perpetual verdure, unimpaired by either ſolſtice. Hence too the farmer is enabled to lay his lands under graſs almoſt at any ſeaſon, even at the commencement of winter ‡: and hence the grazier never loſes the benefit of his rich paſtures at any period of the year, unleſs during the tranſient paſſage of a temporary drift of ſnow; ſo that horſes, cattle, and ſheep, arrive here, with little care, at a degree of perfection unattainable in other countries without infinite expence and trouble.

For the growth of flax, the ſtaple commodity of the kingdom; of potatoes, the general food of its inhabitants; and of barley, from whence the ardent ſpirits and other fermented liquors of the country are derived; the climate ſeems to poſſeſs a peculiar felicity of tem-

* See Hamilton's paper on the temperature of Ireland, in the ſecond volume of theſe Tranſactions.

† See State of the Weather in Dublin, from June 1791 to June 1793, by Mr. Kirwan, Vol. V. of theſe Tranſactions.

‡ See Young's Tour in Ireland, Vol. I. p. 130.

perature,

perature, and moist in all its seasons; and for their necessary degree of ripeness the heat of summer is amply sufficient.

It is for the more delicate species of garden-fruits, for ill conducted crops of oats in bleak and mountainous situations, and for the complete and perfect maturity of wheat, that uneasy apprehensions can ever be entertained by the gardener or the farmer.

The former are in themselves of little importance; and, where such luxuries are required, glass coverings and artificial heat afford an ample supply, and remove all pretence for discontent. Human art and industry, attentive to circumstances, and accommodated to times, has stolen from the mild temperatures of our winters as much warmth as serves to compensate for the deficiency of summer heats in forwarding and perfecting the crops of wheat; and a little will teach the farmer in our mountainous provinces to use the same artifice with respect to his oats, whenever necessity shall demand his increased attention.

To sum up matters then with truth and brevity—A density of population, surpassing that of the vaunted millions of undepopulated France *; a copious export-trade in provisions of various kinds, unequalled by any kingdom whose inhabitants are proportionably numerous †; and a staple manufacture, unrivalled in general use, in certainty of produce, and intrinsic value ‡; are circumstances which have not fallen to the lot of other nations, and bring with them clear and irrefragable evidence to demonstrate a salubrious country, a genial climate, and a fertile soil in Ireland.

* The inhabitants of Ireland amount to more than four millions. (See Mr. Bushe's Memoir, Irish Trans. Vol. III.)

Hence the density of its population is at the rate of 182 persons to a square geographical mile, supposing the island to contain in round numbers 22,000 superficial miles.

Mr. Zimmerman, in his Political Survey of Europe, erroneously supposing the inhabitants of this country less than two millions and a half, deduces a density of population only to 117 persons on each mile. The following table marks the number of individuals on a geographical square mile in the northern countries of Europe, as taken from Zimmerman's tables (Ireland being corrected); from whence it will appear that this kingdom rates extremely in the density of its population.

	Persons.
Denmark - - -	12
Sweden - - -	14
Russia - - -	20
Scotland - - -	51
Poland - - -	53
Prussia - - -	67
England - - -	129
Germany - - -	135
France - - -	152
Ireland - - -	182
Holland - - -	236

† Two hundred and twenty thousand barrels of beef and pork, independent of the various matters connected therewith, and of every other species of provision, were exported in the year ending 25th March 1793.

‡ After a progressive increase of ninety years the exports of linen-cloth alone, in the year 1792, amounted to forty-three million yards, from one million only at the commencement of the present century.

Conclusion.

IN this Memoir I have endeavoured to prove, from natural and almost incontrovertible registers of the phenomena of later years, that the winds, and particularly the western storms, have swept with increased violence over Ireland.

From this fact I have deduced a necessary change in its climate; a more general equability of temperature through the year; summers less warm, and winters more mild and open: and lastly, I have endeavoured to support this conclusion by general observation, and the enumeration of particular instances where the defect of summer heat, and winter ice, seem to be the most strongly marked.

One question still remains curious in its principle, and interesting in its solution—Why have these western winds blown with unusual and increased violence?

The limits of a Memoir such as this do not permit me to enlarge on this subject, and even appear to include a reproof for the length of the present interruption. I shall therefore trespass no longer on the moments of the Academy than to suggest a few queries, which may stand over for future consideration; leaving it to time, and the ingenuity or better directed observations of others, to verify, to disprove, or to condemn, the hints which may be contained in them.

1st. Have not our winds become more violent, and the temperature of our seasons more equable, since the forests of Ireland were cleared, and the country cultivated? And have not these winds, and that equability of temperature, been nearly proportioned to these as to their causes?

2d. Have not similar changes occurred under analogous circumstances in North America; even in Canada, that country of extremes in heat and cold? And did not the island of Bermudas, though situated so much to the southward of us, become barren of fruit in consequence of the destruction of its timber-trees?

3d. Has it not appeared from observations on the ascent of balloons, and the motion of clouds, that the lower mass of air often pursues a different course from the upper stratum? May not then the limits of our stormy currents of air be often confined within a few hundred yards of the surface of the earth? And if so, is it not possible, and even probable, that the frequent interruption of forests and groves, and hedge-row trees, might have formerly very much retarded, and finally checked, the progress of a tempest?

4th. Have not all the countries of Europe, Asia, and America, within the parallel of Ireland, been very much denuded of their forests within the present century? And has not the increased velocity of the westerly winds been proportioned to this destruction of the forests and trees, as to their natural causes?

5th. Is it not probable, since the prevalent winds of our parallel have a westerly tendency, that circumstances which have removed impediments to their career round the entire globe would give increased velocity to their course?

6th. Should not Ireland, launched as it is into the Atlantic beyond the other coasts of Europe, and denuded as it has been of forests and hedge-row trees, be most sensibly affected by increasing tempests from the west, and the first to experience their influence on its climate?

Whether these queries have any foundation in nature, or are merely to be esteemed the result of an unrestrained imagination; whether so diminutive an animal as man, so temporary

porary in duration, ſo impotent in ſtrength, acting through the lengthened period and perſevering efforts of a large portion of his ſpecies, can reaſonably be deemed equal to the involuntary production of ſuch vaſt effects; to a change even of the elements and climates of the earth, may admit of doubt, of oppoſition, and even of denial. For theſe reaſons I have taken the liberty ſimply to propoſe them as matters of enquiry; and only beg the indulgence of the Academy for troubling them with matters that may not be attended with ſufficient evidence to bring conviction, or even to eſcape cenſure.

POSTSCRIPT.

October 20, 1795.

THE preſent year (1795) has been comparatively cold in its commencement, and warm in its ſummer: it has been calm, dry, and in many reſpects a contraſt to the long ſeries of preceding years, whoſe effects are regiſtered in this Memoir.

Yet the extremes of its temperature have not been equal to thoſe of former years, as far as can be inferred from the recorded effects of heat and cold on natural objects. The large rivers of the northern province have not been bound firmly in the icy chains of winter, nor have the grapes ripened in favourable aſpects in any part of the kingdom during ſummer, as formerly occurred in ſeaſons whoſe temperature was denominated extreme.

III.

Experiments and Obſervations on Electricity—Excitation—the two States—Points of Difference between the Action of weak and ſtrong Electricities compared together. (*W. N.*)

THE excitation of electrics by friction ſtill remains a myſtery. We poſſeſs nothing in the leaſt reſembling a theory of the changes of electrical capacity which the rubber and cylinder undergo during their change of relative poſition in contact. In the year 1789 I communicated ſeveral facts to the Royal Society* concerning the mutual action of the ſilk flap and the electrical cylinder upon each other, and ſhewed that ſomething like compenſation takes place while they remain together. The tranſition of the ſurface of the cylinder from the compenſated to the uncompenſated ſtate is ſtrikingly ſeen when a hole is cut in the ſilk, and the cylinder turned under circumſtances of conſiderable excitation. A caſcade of fire iſſues from that edge of the hole which is neareſt the cuſhion; but inſtead of being diſſipated into the air, it bends down again, and unites with the cylinder at the oppoſite edge of the hole, whence it proceeds as uſual to the receiving part of the prime conductor.

The phenomena of the two electricities called plus and minus are ſingularly diſtinct in almoſt every experiment which can be made with the exhibition of electric light. Paper is a good ſubſtance for obſerving the viſible paſſage of electricity. If a ſtrong plus electric ſtream be let fall on the flat ſide of an uninſulated ſheet of paper, it forms a beautiful ſtar about four inches in diameter, conſiſting of very diſtinct radii not ramified. The minus electricity, in circumſtances perfectly ſimilar, throws many pointed bruſhes to the paper,

* Philoſophical Tranſactions, No. lxxix.

but

but forms no ftar upon it. In this experiment I ufed a machine with a cylinder of feven inches diameter.

Hence it feems to follow, that a hollow ball of paper, or a glafs globe covered with paper, might form an amufing part of the electrical apparatus for experiments in the dark.

The laws of action with regard to weak electricities and thofe of confiderable intenfity appear to differ in various particulars, which require further inveftigation. When a number of jars are charged in fucceffion, that is to fay, by infulating the whole feries, and caufing the outer coating of the firft to communicate with the inner of the fecond, and the outer coating of the fecond with the inner of the third, and fo forth, it is well known that the whole may be charged by communicating electricity to the infide of the firft, provided the laft have a communication with the earth; but with this circumftance, that the charge will reftore itfelf by explofion when the quantity of electricity is much lefs than the firft jar would fingly have received and retained. From this refult it has with fome probability been concluded, that glafs refifts the communication of electric energy, and that the jars are fucceffively lefs and lefs charged. Whether this laft conclufion be true, has not I believe been afcertained by experiment.

Under all the uncertainties concerning the place occupied by the electric charge of coated glafs, though it may feem unfair to make any inference refpecting glafs which is uncoated, yet, upon the whole, there appears to be a probability that the interpofition of naked glafs may impede the action of electrified bodies. This queftion more immediately points at the tube in which the gold-leaf electrometer of Bennet is inclofed. To determine whether the tube of the electrometer does affect the electric ftate of the included leaf, either by compenfation or otherwife, I took a piece of window-glafs eighteen inches long, two inches wide, and one-twentieth of an inch thick, which I cleaned very well, and then paffed it feveral times through the hot air over the flame of a candle. In this ftate one end of the glafs was laid gently upon the electrified plate of Bennet's electrometer, and then fuddenly raifed by a turn of the wrift. It was fcarcely poffible to difcern that the leaves were at all affected; but when the electrometer was in the plus ftate a very flight collapfion was produced by raifing the glafs, and the contrary effect was produced when the electrometer was negative. Some days afterwards the experiment was repeated, after the gold-leaf had been changed for other pieces, which were very pointed and delicate in their movements. The refult was, that the glafs was always fhewn by the electrometer to be in a weak pofitive ftate; and, when the electricity of the electrometer was made plus, the collapfion was equal to the divergence when it was minus.

In making thefe experiments I had previoufly fuppofed that the influence of the metallic ftate of the electrometer would produce fomewhat of the nature of a charge upon the glafs; and confequently that the intenfity of the leaves would have been diminifhed during the exiftence of that charge; and alfo, that in fuch a cafe the action of the metal through the glafs would be fubject to the fame diminution as in the feries of jars. But as the glafs did not appear to act in this manner, it feems proper to conclude that clean glafs does not affect the electric ftate of bodies by its vicinity, and that the divergence of the balls or the gold-leaf in the electrometers of Cavallo and Bennet is not diminifhed by the tube which furrounds them.

 From

From a variety of experiments it was clearly aſcertained that the metallic coatings, though by their vicinity they may diminiſh the intenſity of the electric ſtate in the leaves, do nevertheleſs increaſe the angle of divergence by their attraction.

When the gold-leaf electrometer is made with a very ſmall tube, its ſenſibility is ſomewhat increaſed by the nearneſs of the coatings; but the chance of rendering it unſerviceable from caſual friction, which excites the glaſs, and cauſes the gold leaf to ſtick to it, together with the leſs perfect view of the divergence through a tube of ſmall curvature, afford reaſons why a diameter of leſs than an inch ſhould be rejected. Other reaſons of convenience indicate that the diameter of the glaſs ſhould not much exceed this quantity.

I was once induced to think that the conſiderable magnitude of the cap of Bennet's electrometer might render it leſs capable of being acted upon by ſmall quantities of electricity. Experiment did not however give much countenance to this ſuppoſition. By trials with heads of different ſize, the ſmalleſt were found to be rather more ſenſible to extremely minute electricities, and leſs ſo to ſuch as were greater. The influence of very weak electricity may produce the oppoſite ſtate in the whole of a ſmall head, but only in part of a larger; the remaining part of this laſt aſſuming the oppoſite ſtate, and robbing the leaves of part of their intenſity. But in higher electricities the whole of the large head may be urged to give electricity to the leaves, in a quantity which the ſmaller head could not give without acquiring a higher degree of intenſity, and conſequently more ſtrongly reſiſting the deſired proceſs. It appears therefore that the maximum of effect with a given electricity, acting without communication, will not be obtained but by an head of a definite figure and magnitude.

From ſome experiments of Hoadley and Wilſon with a number of prime conductors ſeparately inſulated, it was eſtabliſhed that an electrified body brought near one end of ſuch a ſet of conductors, forming by contact a right line, will produce the contrary ſtate in the nearer conductors, and the ſame ſtate in thoſe which are more remote. Or, upon the hypotheſis of a ſingle fluid, the electricity of the body preſented will repel that of the compound conductor; ſo that by ſeparating them before the influence is removed, and afterwards examining them, the reſpective ſtates of the ſeveral portions of the whole line may be aſcertained.

It was a concluſion obviouſly enough deducible from this experiment, that if two electrometers of Bennet were connected by a metallic bar, there ought to be a difference when an electrified body is preſented at either extremity, or at the middle of the bar. I made this experiment with a braſs bar eighteen inches long. Both electrometers were affected at the ſame inſtant. Their divergence was equal, and the effect was the ſame, whether the glaſs or ſealing-wax was preſented at either end or at the middle of the bar. This reſult ſeems to indicate a difference in the mode of action between the very weak electricity I uſed and the much ſtronger of Hoadley and Wilſon.

The action of pointed bodies and of flame likewiſe exhibits a remarkable difference between the ſtrong and the weak electricities. The prime conductor of an electrical machine is very ſuddenly deprived of the greateſt part of its electricity by the operation of a metallic point; but does not ſeem to be much affected by the vicinity of burning

candles.

candles. On the contrary, the electrometer of Bennet will ſcarcely communicate the leaſt portion of its electricity to a point not abſolutely in contact with its cap; but is readily deprived of its electric ſtate by a candle.

Coulomb, by his method of torſion, has determined that the action of weak electricities diminiſhes as the ſquare of the diſtance. I do not know that any experiments have been made to determine the ratio of the repulſion or attraction of bodies by electricity, with regard to the diſtance, when the intenſities are conſiderable.

To examine this object I cauſed a braſs conductor to be made, four inches in diameter, with a ſpherical part at each end, of five inches diameter; and, on the whole, twenty inches long. It was fixed with its axis in the vertical poſition. In the upper ſpherical part there was a ſtage ſupporting a pulley, the axis of which ran upon two pair of very delicate friction wheels. The lower ſpherical part was perforated in the axis, to receive the ſtem of a very light ſphere of paper gilt, and of the diameter of five inches. Care was taken that this ſtem, which was nearly the whole length of the conductor from the friction wheels to the lower orifice, ſhould not touch that orifice; and for greater ſecurity there was a ſmall frame carrying three friction rollers, againſt one of which it was ſure to bear in caſe of accidental irregularity. The upper part of the ſtem was faſtened to a fine ſilken thread which paſſed over the pulley, and was tied to a counterpoiſe adapted by its figure to receive ſmall weights, in order that the equilibrium or preponderance might be adjuſted at pleaſure. On the axis of the pulley was an index and hand, which moved upon a face without touching, beneath a convex glaſs, and ſhewed the deſcent of the gilded ball in inches and parts. I expected to have obtained a conſiderable ſcale on my ſtem of twelve inches. But in the trials I made when it was finiſhed, in which the aſcending power of the ball could be varied at pleaſure by addition to the counterpoiſe, I found either that it did not move at all, or that, if it did, it ran with conſiderable velocity through its whole length. This unexpected event, together with ſome other imperfections in the inſtrument itſelf, which required amendment and alteration, prevented me from purſuing and diverſifying a courſe of experiment that promiſed ſo little. The facts appear however to ſhew, that in great intenſities and ſhort diſtances, the diminution of effect, if it follow the law of the ſquares of the diſtances, is, like the attraction of the earth upon projectiles, too ſmall to be perceived. But from the indications afforded by Henley's quadrant electrometer, and the floating electrometer of Nollet, there appear to be reaſons why the purſuit ſhould not be thought unworthy of further experiment.

IV.

Analyſis of the Red Lead of Siberia; with Experiments on the New Metal it contains. By Citizen VAUQUELIN, Inſpector of Mines, and Conſervator of Chemical Products at the Mineralogical School.

(Concluded from page 393, Vol. II.)

Combinations of the Acid of Red Lead with the Alkalis.

THE acid of red lead forms, in its combination with the alkalis, ſalts which are ſoluble, cryſtalliſable, and coloured. The ſimpleſt proceſs for making theſe ſalts conſiſts

in

in boiling upon one part of red lead reduced into fine powder, two parts of carbonate of alkali with forty parts of water. By this means a double affinity is made to operate, by virtue of which carbonate of lead is formed, and falls to the bottom, while a combination of the acid of red lead with the alkali made ufe of remains fufpended in the water, by virtue of its folubility.

The proportion of alkaline carbonate proper to be ufed for the decompofition of the native red lead muft vary according to the proportions which fubfift between the bafes, the acids, and the water of cryftallization. The proportions ftated in the laft paragraph are fuited to the carbonate of pot-afh; but the proper quantities in the other cafes are eafily afcertained, by ufing a fmall quantity of the carbonate at firft, and afterwards gradually increafing it, till the greateft part of the red lead is decompofed. On the whole, it is better to ufe a fmaller than too large a portion of the carbonate, in order that its bafe may be more completely faturated with the acid of the red lead.

There is no reafon to fear a fimilar inconvenience with the carbonate of ammoniac. It is proper, on the contrary, to add more than is fufficient to faturate the acid of the red lead; becaufe a portion of this falt will always be driven off during the ebullition which is indifpenfably neceffary to produce the intended decompofition. And even in cafe there fhould remain an excefs after the complete decompofition of the red lead, there would be no danger, becaufe this excefs of alkali is driven off by the evaporation of the falt newly formed, and there is always a certainty of obtaining the combination perfectly pure.

The colour of the combinations of the acid of red lead with the alkalis is an orange yellow *. Their folutions afford cryftals nearly of the fame tinge, the differences being only flight variations of fhade. Thefe falts are decompofed by barytes, lime and ftrontian. The mineral acids likewife decompofe them by an inverfe operation.

Thefe falts afford oxygen gas by the action of heat, and their refidue has the appearance of a green mafs. The ammoniacal compound muft however be excepted, as its bafe is partly decompofed by the oxygen of the acid, and leaves in the retort a pure green oxide, the undecompofed portion of ammoniac being alfo driven off by the heat.

Thefe falts decompofe by double affinity the calcareous, barytic, magnefian, aluminous falts, &c. Moft of the metallic falts are likewife decompofed by thefe fubftances, and afford new combinations fcarcely or not at all foluble in water; and for the moft par red, yellow, orange, or lemon colour.

I have not hitherto been able to determine the figure of thefe falts with accuracy nor their degree of folubility in water, for the reafons I have already explained; neither have I made all the combinations of this acid with the metallic oxides, nor determined its mode of action on the metals. Such experiments would no doubt have afforded interesting phenomena for the completion of the hiftory of this metallic acid.

But I indulge the hope that thefe experiments are only retarded, and that fome fortunate opportunity will hereafter enable us to perform them: and, at all events, the experiments already made will be fufficient to afcertain the prefence of the acid of red lead wherever it may be met with, and diftinguifh it from every other natural fubftance.

* That of the ammoniacal falt, which has the form of plated cryftals, is yellow, and exhibits the metallic brilliancy of gold. V.

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SECTION VII.

Reduction of the Acid of Red Lead to the Metallic State.

THOUGH the properties of the acid of red lead, deſcribed in the foregoing paragraphs, are in ſtrictneſs ſufficient to convince thoſe who are in the habit of treating metallic ſubſtances, that this ſubſtance belongs to the ſame claſs, it is nevertheleſs deſirable, in order, as it were, to add the ſtamp of evidence to the proofs of demonſtration, that this acid ſhould be reduced to the metallic ſtate.

To obtain this object I took 72 parts of the acid in queſtion, extracted from the red lead by the muriatic acid, as deſcribed in our fourth ſection, which I introduced into a crucible of charcoal placed in another of hard porcelain likewiſe filled with charcoal powder.

This apparatus, placed in a forge furnace, was expoſed for an hour to the action of a very ſtrong fire urged by the blaſt of a ſtrong pair of bellows with three pipes.

When the crucible was cooled and broken, I found, to my great ſatisfaction, in the ſmall veſſel of charcoal, a metallic maſs of a white grey colour, ſhining, very brittle, and upon the ſurface of which were many feathered cryſtals of the ſame colour perfectly metallic. This metallic maſs weighed forty-three parts.

It appears, from the reſult of this operation, that the oxygen does not adhere with very great force to this metallic baſe, and that the acid contains about 0,40 of its weight of this acidifying principle.

SECTION VIII.

Properties of the Metal of Red Lead.

THIS metal, as I have already remarked, is of a white colour inclining to grey: it is very brittle, and cryſtalliſes in an elevated temperature into a kind of feathered filaments, which riſe above the metallic maſs.

The metallic button being broken, preſented internally compact points forming a cloſe grain, and in other parts needles croſſing each other in all directions, leaving void ſpaces between them, which prevented me from determining its ſpecific gravity.

A fragment of this metal expoſed to the heat of the blow-pipe acquired a tarniſh on its ſurface, and by continuing the operation it was covered with a light green cruſt; but it afforded no ſign of fuſion.

When heated by the ſame apparatus with borax it did not melt, but was in a ſmall degree diminiſhed, and communicated an emerald-green colour to the ſalt.

When reduced to a fine powder, and treated with the concentrated boiling nitric acid, it was oxided with much difficulty, and communicated to the acid a green tinge ſlightly inclining to blue. The nitric acid attacks this metal with ſo much difficulty, that it was not till after treating it repeatedly with conſiderable quantities of that ſolvent that I could ſucceed in diſſolving ſix grains.

The different ſolutions being added together were evaporated to dryneſs. Towards the end of the operation the reſidue aſſumed the form and the ductility of a vegetable extract of a red brown colour. Cauſtic potaſh poured on this reſidue diſſolved a great part, and aſſumed a lemon-yellow colour; but part remained of a very beautiful green, which was not acted upon by that re-agent: it was a portion of the metal which had not received

from the nitric acid the whole quantity of oxygen neceſſary for its acidification. It was ſtill in the ſtate of oxide.

I treated it again with the concentrated nitric acid; and by a ſeries of operations of this nature I at length ſucceeded in completely acidifying it.

The combination of this artificial acid with potaſh exhibited abſolutely the ſame appearances with the different re-agents as the ſalt which is formed with the natural acid. See Section II.

SECTION IX.

Denomination of the Metal contained in the native Red Lead.

WHEN an unknown ſubſtance is to be examined, the only method to aſcertain whether it has been before deſcribed, is to examine its properties, and compare them with thoſe of other bodies; an operation which ſuppoſes a knowledge of all that has been before deſcribed in natural hiſtory. And when, after an accurate compariſon of the properties of the body under examination with thoſe of other bodies, it is found that none of theſe laſt exhibit the whole of thoſe properties, a fair concluſion may be formed that the body is unknown, and conſequently that it is new.

After this point is determined, it becomes neceſſary, in order to make it known to others, that its diſtinctive characters ſhould be clearly aſcertained, and a name given to it, for the purpoſe of brief deſignation, and of inſcription in the catalogue of human knowledge.

This name may be derived from various ſources;—the place whence it was obtained, the author who diſcovered it, or the ſpecific properties it poſſeſſes, &c.: but it is eaſy to ſee that the two firſt ſources are vicious. In fact, the name of the place ſeems to announce that the ſubſtance is to be found excluſively there; and it is very far from being known, at the early period of diſcovery, whether it may not be found elſewhere. The name of the author teaches nothing, except that ſuch a man was the diſcoverer; a circumſtance of little intereſt to ſcience. But the name of a ſubſtance deduced from its leading properties is truly uſeful, becauſe it brings to mind, and in ſome meaſure places the object before the eyes by a faithful ſketch of its attributes.

From theſe conſiderations I have thought fit to adopt the name *chrome*, which was propoſed to me by Cit. Haüy, to deſignate the new metal found in the native red lead. In truth, this name does not perfectly agree with the complete metal, becauſe it has no very diſtinct colour; and becauſe, even if it had one, this would not be a ſufficient reaſon, ſince every metal has a more or leſs peculiar colour.

But it agrees wonderfully well with its combinations with oxygen, which afford a green oxide, or a red acid, according to the proportions of that principle, and becauſe each of its primary combinations communicates its colour to all the ſecondary combinations into which it enters; properties which belong to it almoſt excluſively.

This name appears to me to be ſo much the better founded, as this ſubſtance has not yet been diſcovered except in the ſtate of acid or oxide, and becauſe it may perhaps never be found in the native metallic ſtate.

Nevertheleſs, as I am not diſpoſed to conſider the adoption of one name rather than another as of any importance, provided the name do expreſs ſome remarkable or diſtinctive property of that which it denotes, I ſhall with pleaſure, if a better name be found, conſent to ſubſtitute it inſtead of *chrome*.

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SECTION X.

Uſes of Chrome, of the Oxide of Chrome, and of the Chromic Acid.

THE brittleneſs of chrome, its reſiſtance to the action of fire, and the ſmall maſſes in which it has hitherto been found, afford little expectation that this metal will ever be of great uſe in the arts. But this aſſertion may be too ſtrong; for a new ſubſtance, of which the properties at firſt appear to be of ſmall intereſt to ſociety, is ſometimes found, in the ſubſequent proceſſes of inveſtigation, to be capable of very important applications in the arts and ſciences.

The acid and the oxide of this metal are certainly not ſubject to the ſame obſervation. The former, by the beautiful emerald-green which it communicates even to enamels, without being ſubject to alteration in the purity of its ſhade, will afford to painters and enamellers an additional object to enrich their productions, and add to the perfection of their art: the ſecond, by the beautiful cinnabar red which it aſſumes and keeps in its combination with mercury; the orange-red colour it affords with lead; the carmelite red it communicates to ſilver, may become highly valuable for paintings in oil and water-colours.

Chemiſtry will be greatly benefited by this excellent re-agent to detect the ſmalleſt quantities of mercury, ſilver, and lead, diſſolved in acids by the different colours it produces when its action is aſſiſted by an alkali. And, in the ſame manner as this acid indicates the preſence of the metals here ſpoken of, thoſe metals in their turn may ſerve to diſcover the chromic acid, if it be previouſly put into the neceſſary condition to produce the before-mentioned effects.

If the chromic acid ſhould hereafter be abundantly found in any other combination beſides that of lead, it might, after extraction by carbonate of potaſh, be uſed for the artificial compoſition of red lead, and furniſh, in abundance, this valuable orange-red colour for painters' uſe, which is ſold very dear in Siberia, and applied to this purpoſe with great ſucceſs. For this object the native red lead, which is diſſeminated in ſmall cryſtals, or plates, in the fiſſures of quartzoſe or gritſtone gangues, in which it is commonly found, might be pulverized, then boiled with a ſolution of carbonate of potaſh, and mixed with nitrate of lead; the acid of which will ſaturate the potaſh, and afford its metal to the new acid, which will thus produce a colour no leſs beautiful than the natural, and perfectly clear of the matrix.

There is reaſon to preſume that chrome, either in the ſtate of oxide or of acid, will be found ſingly or engaged in ſome other combinations. For the analyſis of the emerald of Peru has already ſhewn me that its colouring part is afforded by the oxide of this metal; a circumſtance which gives the moſt agreeable proof of the goodneſs and fixity of this colour, as it is known that the emerald can ſuſtain the moſt violent degree of heat without loſing its colour.

I have likewiſe found that the yellowiſh green tufted cryſtals, which often accompany the red lead ore of Siberia, are formed of chrome and lead, both united, in the ſtate of oxide. Green cryſtals poſſeſſing the ſame form, the ſame dimenſions, and the ſame ſituations on the matrix as thoſe of red lead, but which are a combination of the oxides of chrome and of lead, are likewiſe found in certain ſpecimens of the native red lead. It is probable that theſe combinations originally exiſted in the ſtate of chromate of lead, and that in proceſs of

of time a portion of oxygen having been carried off by unknown causes, converted them to the state of oxide, and changed the red colour to green.

From paragraph III. it follows that the Siberian red lead contains

	By analysis. Experiment V.	By synthesis. Experiment VI.
Oxide of lead - -	63,96	65,12
Chromic acid - -	36,40	34,88

V.

Experiments to determine the Density of the Earth. By HENRY CAVENDISH, *Esq. F. R. S. and A. S.**

MANY years ago the Rev. John Michell, F. R. S. contrived a method of determining the density of the earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian professor at Cambridge, who, not having conveniences for making experiments with it in the manner he could wish, presented it to Mr. Cavendish.

The apparatus is very simple: it consists of a wooden arm six feet long, made so as to unite great strength with little weight. This arm is suspended in an horizontal position by a slender wire forty inches long, and to each extremity is hung a leaden ball about two inches in diameter; and the whole is inclosed in a narrow wooden case, to defend it from the wind.

As no more force is required to make this arm turn round on its centre than is necessary to twist the suspending wire, it is plain that, if the wire is sufficiently slender, the most minute force, such as the attraction of a leaden weight a few inches in diameter, will be sufficient to draw the arm sensibly aside. The weights which Mr. Michell intended to use were eight inches diameter. One of these was to be placed on one side of the case, opposite to one of the balls, and as near it as could conveniently be done, and the other on the other side, opposite to the other ball, so that the attraction of both these weights would conspire in drawing the arm aside; and when its position, as affected by these weights, was ascertained, the weights were to be removed to the other side of the case, so as to draw the arm the contrary way, and the position of the arm was to be again determined; and consequently half the difference of these positions would shew how much the arm was drawn aside by the attraction of the weights.

In order to determine from hence the density of the earth, it is necessary to ascertain what force is required to draw the arm aside through a given space. This Mr. Michell intended to do by putting the arm in motion, and observing the time of its vibrations, from which it may be easily computed†.

* Abridgment of his paper in the Philosophical Transactions, 1798, p. 469.

† Mr. Coulomb has in a variety of cases used a contrivance of this kind for trying small attractions; but Mr. Michell informed Mr. C. of his intention of making this experiment, and of the method he intended to use, before the publication of any of Mr. Coulomb's experiments.

Mr. Michell had prepared two wooden ſtands, on which the leaden weights were to be ſupported and puſhed forwards till they came almoſt in contact with the caſe; but he ſeems to have intended to move them by hand.

As the force with which the balls are attracted by theſe weights is exceſſively minute, not more than one fifty-millionth part of their weight, it is plain that a very minute diſturbing force will be ſufficient to deſtroy the ſucceſs of the experiment: and from the following experiments it will appear, that the diſturbing force moſt difficult to guard againſt is that ariſing from the variations of heat and cold; for, if one ſide of the caſe is warmer than the other, the air in contact with it will be rarefied, and in conſequence will aſcend, while that on the other ſide will deſcend, and produce a current which will draw the arm ſenſibly aſide *.

As Mr. Cavendiſh was convinced of the neceſſity of guarding againſt this ſource of error, he reſolved to place the apparatus in a room which ſhould remain conſtantly ſhut, and to obſerve the motion of the arm from without by means of a teleſcope, and to ſuſpend the leaden weights in ſuch a manner that he could move them without entering into the room. This difference in the manner of obſerving rendered it neceſſary to make ſome alteration in Mr. Michell's apparatus: and as there were ſome parts of it which Mr. Cavendiſh thought not ſo convenient as could be wiſhed, he choſe to make the greateſt part of it afreſh.

Plate XIX. is a longitudinal vertical ſection through the inſtrument, and the building in which it is placed. A B C D D C B A E F F E is the caſe, *x* and *x* are two balls which are ſuſpended by the wires *h x* from the arm *g h m h*, which is itſelf ſuſpended by the ſlender wire *g l*. This arm conſiſts of a ſlender deal rod *h m h*, ſtrengthened by a ſilver wire *h g h*; by which means it is made ſtrong enough to ſupport the balls, though very light †.

The caſe is ſupported and ſet horizontal by four ſcrews, reſting on poſts fixed firmly into the ground: two of them are repreſented in the figure by S and S; the two others are not repreſented, to avoid confuſion. G G and G G are the end walls of the building. W and W are the leaden weights which are ſuſpended by the copper rods R*r* P*r*R and the wooden bar *r r* from the centre pin P*p*. This pin paſſes through a hole in the beam H H, perpendicularly over the centre of the inſtrument, and turns round in it, being prevented from falling by the plate *p*. M M is a pulley faſtened to this pin, and M*m* a cord wound round the pulley and paſſing through the end wall, by which the obſerver may turn it round, and thereby move the weights from one ſituation to the other. When the weights are in the poſition repreſented in the engraving, both conſpire, by their action on the ſmall weights *x x*, to draw the arm in the ſame direction; but when, by means of the pulley

* Mr. Caſſini, in obſerving the variation-compaſs placed by him in the Obſervatory (which was conſtructed ſo as to make very minute changes of poſition viſible, and in which the needle was ſuſpended by a ſilk thread), found that ſtanding near the box, in order to obſerve, drew the needle ſenſibly aſide; which I have no doubt was cauſed by this current of air. It muſt be obſerved that his compaſs-box was of metal, which tranſmits heat faſter than wood, and alſo was many inches deep; both which cauſes ſerved to increaſe the current of air. To diminiſh the effect of this current, it is by all means adviſable to make the box in which the needle plays not much deeper than is neceſſary to prevent the needle from ſtriking againſt the top and bottom. C.

† Mr. Michell's rod was entirely of wood, and was much ſtronger and ſtiffer than this, though not much heavier; but as it had warped when it came to Mr. Cavendiſh, he choſe to make another, and preferred this form, partly as being eaſier to conſtruct and meeting with leſs reſiſtance from the air, and partly becauſe, from its being of a leſs complicated form, he could more eaſily compute how much it was attracted by the weights.

MM, the observer causes them to move through a considerable portion of a semicircle, they will arrive at a second position, in which their joint action will move the arm in the contrary direction. These weights are prevented from striking the instrument by pieces of wood fastened to the wall of the building.

The situation of the arm is determined by slips of ivory placed within the case near each extremity of the arm. These slips bear a set of divisions of the inch into twenty parts; to which a small slip at each end of the arm applies, without contact, a vernier that affords a subdivision into hundredth parts. Estimation by the eye looking through the short telescopes T and T, gives the position of the arm to still greater nicety. The lamps L and L throw light on the divisions by convex glasses, and no other light is admitted into the room.

FK is a wooden rod, which, by means of an endless screw, turns round the support to which the wire *g l* is fastened, and thereby enables the observer to turn round the wire till the arm settles in the middle of the case without danger of touching either side. The wire *g l* is fastened to its support at top, and to the centre of the arm at bottom, by brass clips, in which it is pinched by screws. The different parts are drawn nearly in the proper proportion to each other.

Suppose the arm to be at rest and its position to be observed, let the weights be then moved; the arm will not only be drawn aside thereby, but it will be made to vibrate, and its vibrations will continue a great while; so that, in order to determine how much the arm is drawn aside, it is necessary to observe the extreme points of the vibrations, and from thence to determine the point which it would rest at if its motion were destroyed, or the point of rest. To do this, Mr. Cavendish observes three successive extreme points of vibration, and takes the mean between the first and third of these points as the extreme point of vibration in one direction, and then assumes the mean between this and the second extreme as the point of rest; for, as the vibrations are continually diminishing, it is evident that the mean between two extreme points will not give the true point of rest.

It may, he adds, be thought more exact to observe many extreme points of vibration, so as to find the point of rest by different sets of three extremes, and to take the mean result; but it must be observed, that, notwithstanding the pains taken to prevent any disturbing force, the arm will seldom remain perfectly at rest for an hour together; for which reason it is best to determine the point of rest from observations made as soon after the motion of the weights as possible.

The next thing to be determined is the time of vibration, which he finds in this manner: He observes the two extreme points of a vibration, and also the times at which the arm arrives at two given divisions between these extremes, taking care, as well as he can guess, that these divisions shall be on different sides of the middle point, and not very far from it. He then completes the middle point of the vibration, and by proportion finds the time at which the arm comes to this middle point. He then, after a number of vibrations, repeats this operation, and divides the interval of time between the coming of the arm to these two middle points by the number of vibrations, which gives the time of one vibration. The propriety of this method is more fully explained in the Memoir itself.

In Mr. Cavendish's first experiments the wire by which the arm was suspended was $39\frac{1}{4}$ inches long. It was of copper silvered, one foot of which weighed $2\frac{4}{10}$ths grains; and its stiffness was such as to make the arm perform one vibration in about 15 minutes. It was immediately

immediately found that it was not ſtiff enough, as the attraction of the weights drew the balls ſo much aſide as to make them touch the ſides of the caſe. It was thought proper, however, to make ſome experiments before a ſtiffer wire was ſubſtituted in its place.

In this trial the rods by which the leaden weights were ſuſpended were of iron, the probability of magnetiſm in which ſeemed to be of no conſequence, as the arm contained nothing magnetical. Some trials however, for greater ſecurity, were made with the rods alone. The effect was ſuch as might juſtify a very ſlight ſuſpicion of magnetiſm; for which reaſon the rods were changed for others of copper. When the weights were hung on, the attraction on the ball was very ſenſible, as it carried the latter through fifteen diviſions, or $1\frac{1}{2}$ inch; and the regularity of the reſults was ſuch, that the extremes did not differ from each other more than one-tenth part. It was found that the effect of the attraction ſeemed to increaſe for half an hour or an hour after the motion of the weights; an effect which might be ſuppoſed to ariſe from a want of elaſticity, either in the ſuſpended wire or ſomething it was faſtened to, which might make it yield more to a given preſſure, after a long continuance of that preſſure, than it did at firſt. This ſuſpicion was put to the trial by keeping the wire for two or three hours in a ſtate of torſion amounting to fifteen diviſions, and then returning it by means of the handle K to its original ſituation. It was not found by two repetitions of the experiment that the wire had acquired any ſet or permanent twiſt. The arm was then ſuſpended by a ſtiffer wire, with which the apparatus, inſtead of performing its vibration in about fifteen minutes, employed only ſeven. The motion of the arm was in this caſe not quite ſix diviſions between the two oppoſite poſitions of the weights, or the deviation from the natural ſtation of the arm was half that quantity.

A variation of the power of the weights upon the balls, in ſome reſpect ſimilar to that obſerved with the ſmall wire, was alſo found to take place in the preſent experiments. It happened that the caſe in which the arm was encloſed was placed nearly parallel to the magnetic eaſt and weſt; and therefore, if there was any thing magnetic in the balls and weights, the balls would acquire polarity from the earth—and the weights alſo, after remaining for ſome time in either of the near poſitions, would acquire a polarity of the ſame kind, and attract the balls more ſtrongly on that account; and on the contrary, when removed to the oppoſite near poſition, if the magnetiſm continued permanent in the weights for ſufficient time, the magnetic power would operate in the way of repulſion. This was in fact the kind of effect pointed out by the obſervations. The accurate habits which ſo eminently diſtinguiſh all the experimental proceſſes of Mr. Cavendiſh did not however permit him to reſt contented with the induction to which theſe facts ſeemed to point. He altered his apparatus ſo as to produce at pleaſure a rotation in the weights upon their vertical axes, without opening the room or diſturbing their actual ſituation. By this contrivance he had it in his power to diſpoſe the imaginary magnetic poles in ſuch poſitions as were beſt calculated to detect their operation. The arm was not affected; and conſequently it follows that the irregularity in queſtion did not ariſe from magnetiſm. This concluſion was ſtill more firmly eſtabliſhed by ſubſtituting two actual magnetic bars in the place of the weights; for it was not obſerved that any alteration was produced by reverſing the poſition of their poles.

The next circumſtance that ſuggeſted itſelf was, that the effect might be owing to a difference of temperature between the weights and the caſe; for it is evident, that if the weights

weights were much warmer than the caſe, they would warm that ſide which was next to them, and produce an aſcending current of air, which would be accompanied by a deſcending current towards the oppoſite ſide, and two horizontal currents near the top and bottom; the latter of which would impel the ball towards the weight. Though it ſeemed ſcarcely probable that the weights ſhould happen to be warmer than the caſe, and that in a ſufficient degree to afford any perceptible conſequence, our author neverthelefs determined to ſubmit the matter to examination. He therefore placed two lamps under the weights while in the mid-way poſition, and placed a thermometer with its ball cloſe to the outſide of the caſe, near that part which one of the weights would approach when brought into the poſition to exert its attraction upon the ball. The door of the room was then ſhut, and ſome time afterwards the weights were moved to this laſt-mentioned poſition. At firſt the arm was drawn aſide only in its uſual manner; but in half an hour the effect was ſo much increaſed that the arm was drawn fourteen diviſions aſide inſtead of about three, as it would otherwiſe have been; and the thermometer was raiſed near $1\frac{1}{2}°$, namely from 61° to $62\frac{1}{2}°$. On opening the door, the weights were found to be no more heated than juſt to prevent their feeling cool to the hand.

From the great effect of a difference of temperature it became deſirable to aſcertain the temperature of the weight itſelf, and of the air near the caſe, in the ſubſequent experiments. A ſmall thermometer was therefore inſerted in one of the balls, and another placed cloſe to the caſe. A number of experiments were then made, in which the differences of temperature between the weights and the external air were obſerved; the weights being in ſome inſtances heated by a lamp, and in others cooled by means of ice. It was found that a higher temperature in the weight occaſioned a greater deviation or apparent attraction of the ball; and, on the contrary, that a lower temperature cauſed the ball to be driven by a current in the oppoſite direction.

After this minute and ſcientific inveſtigation of the cauſes which are capable of producing error in the reſults, Mr. Cavendiſh proceeded to make his fundamental experiments. Theſe are tabulated at length in the memoir itſelf, and followed by a detail of the method of computing the denſity of the earth. The firſt thing to be done is to determine from the time of a vibration, what force is required to draw the arm aſide; and the next, to find the proportion which the attraction of the weight upon the ball bears to that of the earth upon the ſame ball, ſuppoſing the ball to be placed in the middle of the caſe: and from theſe reſults (ſince the diameter and ſpecific gravity of the weights and balls compared with water, and the diameter of the earth, are alſo known; together with the facts that the quantities of matter are as the attractive forces at like diſtances, and vary in the inverſe duplicate ratio of the diſtance) the mean denſity of the earth itſelf may alſo be found. Several corrections requiſite to be made in this denſity are then ſtated; namely, on account of the reſiſtance of the arm to motion, the attraction of the weights on the arm, and on the remote ball; the attraction of the copper rods, and of the caſe itſelf; and for the alteration of the attraction of the weights upon the balls according to the poſition of the arm; which laſt is of more ſignification than all the reſt. For all which I muſt, for the ſake of brevity, refer to the Tranſactions.

The

The following TABLE contains the Results of the Experiments.

Experiment	Mot. weight.	Mot. arm.	Do. corr.	Time vibr.	Do. corr.	Density.
				′ ″		
1.	m to +	14,32	13,42		—	5,5
	+ to m	14,1	13,17	14,55	—	5,61
2.	m to +	15,87	14,69	—	—	4,88
	+ to m	15,45	14,14	14,42	—	5,07
3.	+ to m	15,22	13,56	14,39	—	5,26
	m to +	14,5	13,28	14,54	—	5,55
4.	m to +	3,1	2,95	—	6,54	5,36
	+ to —	6,18	—	7,1	—	5,29
	— to +	5,92	—	7,3	—	5,58
5.	+ to —	5,9	—	7,5	—	5,65
	— to +	5,98	—	7,5	—	5,57
6.	m to —	3,03	2,9			5,53
	— to +	5,9	5,71			5,62
7.	m to —	3,15	3,03	7,4	6,57	5,29
	— to +	6,1	5,9	by mean.		5,44
8.	m to —	3,13	3,00			5,34
	— to +	5,72	5,54			5,79
9.	+ to —	6,32	—	6,58	—	5,1
10.	+ to —	6,15	—	6,59	—	5,27
11.	+ to —	6,07	—	7,1	—	5,39
12.	— to +	6,09	—	7,3	—	5,42
13.	— to +	6,12	—	7,6	—	5,47
	+ to —	5,97	—	7,7	—	5,63
14.	— to +	6,27	—	7,6	—	5,34
	+ to —	6,13	—	7,6	—	5,46
15.	— to +	6,34	—	7,7	—	5,3
16.	— to +	6,1	—	7,16	—	5,75
17.	— to +	5,78	—	7,2	—	5,68
	+ to —	5,64	—	7,3	—	5,85

In the second column the letter m denotes the middle position of the weights when each is equidistant from the balls, + denotes one of the near positions, and — the opposite near position.

copy

I copy Mr. Cavendiſh's concluding remarks in his own words:

"From this table it appears, that though the experiments agree pretty well together, yet the difference between them both in the quantity of motion of the arm and in the time of vibration is greater than can proceed merely from the error of obſervation. As to the difference in the motion of the arm, it may very well be accounted for from the current of air produced by the difference of temperature; but whether this can account for the difference in the time of vibration is doubtful. If the current of air was regular, and of the ſame ſwiftneſs in all parts of the vibration of the ball, I think it could not; but as there will moſt likely be much irregularity in the current, it may very likely be ſufficient to account for the difference.

"By a mean of the experiments made with the wire firſt uſed, the denſity of the earth comes out 5,48 times greater than that of water; and by a mean of thoſe made with the latter wire it comes out the ſame; and the extreme difference of the reſults of the twenty-three obſervations made with this wire is only ,75; ſo that the extreme reſults do not differ from the mean by more than ,38, or $\frac{1}{14}$th of the whole, and therefore the denſity ſhould ſeem to be determined hereby to great exactneſs. It indeed may be objected, that as the reſult appears to be influenced by the current of air or ſome other cauſe, the laws of which we are not well acquainted with, this cauſe may perhaps act always or commonly in the ſame direction, and thereby make a conſiderable error in the reſult. But yet, as the experiments were tried in various weathers and with conſiderable variety in the difference of temperature of the weights and air, and with the arm reſting at different diſtances from the ſides of the caſe, it ſeems very unlikely that this cauſe ſhould act ſo uniformly in the ſame way as to make the error of the mean reſult nearly equal to the difference between this and the extreme; and therefore it ſeems very unlikely that the denſity of the earth ſhould differ from 5,48 by ſo much as $\frac{1}{14}$th of the whole.

"Another objection perhaps may be made to theſe experiments, namely, that it is uncertain whether in theſe ſmall diſtances the force of gravity follows exactly the ſame law as in greater diſtances. There is no reaſon however to think that any irregularity of this kind takes place until the bodies come within the action of what is called the attraction of coheſion, and which ſeems to extend only to very minute diſtances. With a view to ſee whether the reſult could be affected by this attraction, I made the 9th, 10th, 11th, and 15th experiments, in which the balls were made to reſt as cloſe to the ſides of the caſe as they could; but there is no difference to be depended on between the reſults under that circumſtance and when the balls are placed in any other part of the caſe.

"According to the experiments made by Dr. Maſkelyne on the attraction of the hill Schehallien, the denſity of the earth is $4\frac{1}{2}$ times that of water; which differs rather more from the preceding determination than I ſhould have expected. But I forbear entering into any conſideration of which determination is moſt to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whoſe quantity I cannot meaſure."

VI. *An*

VI.

An Inquiry concerning the Chemical Properties that have been attributed to Light. By BENJAMIN, *Count of Rumford, F. R. S. M. R. I. A.*

(Concluded from page 405.)

HAVING been fo fuccefsful in my attempts to reduce the oxide of gold, by means of charcoal, *in the moift way*, I loft no time in making fimilar experiments with the oxide of filver.

Experiment No. 11. A folution of fine filver, in ftrong nitrous acid, was evaporated by drynefs, and the refiduum re-diffolved in diftilled water.

A portion of this folution (which was perfectly colourlefs), diluted with twice as much diftilled water, was poured into a phial containing a number of fmall pieces of charcoal; and the phial, being well clofed with a new cork ftopple, was expofed to the action of the fun's rays.

In lefs than an hour fmall fpecks of revived filver began to make their appearance on the furface of the charcoal; and, at the end of two hours, thefe fpecks became very numerous, and had increafed fo much in fize, that they were diftinctly vifible to the naked eye at the diftance of more than three feet. They were very white, and poffeffed the metallic fplendour of filver in fo high a degree, that when enlightened by the fun's beams their luftre was nearly equal to that of very fmall diamonds.

The phial, which was in the form of a pear, and about 1½ inch in diameter at its bulb, was very thin, and made of very fine colourlefs glafs; the aqueous folution was alfo perfectly tranfparent and colourlefs; and, when the contents of the phial were illuminated by the direct rays of a bright fun, the contraft of the white colour of thefe little metallic fpangles with the black charcoal to which they were fixed, and their extreme brilliancy, afforded a very beautiful and interefting fight.

As the air had been previoufly expelled from the charcoal by boiling it in diftilled water, it was fpecifically heavier than the aqueous folution of the metallic oxide, and confequently remained at the bottom of the bottle.

Experiment No. 12. A phial as nearly as poffible like that ufed in the laft experiment, and containing the fame quantity of diluted aqueous folution of nitrate of filver, and alfo of charcoal, was inclofed in a cylindrical tin box, and expofed one hour to the heat of boiling water in an apparatus ufed for boiling potatoes in fteam for the table.

The refult of this experiment was uncommonly ftriking: the furface of the charcoal was covered with a moft beautiful metallic vegetation; fmall filaments of revived filver, refembling fine flatted filver wire, pufhing out from its furface in all directions!

Some of thefe metallic filaments were above one-tenth of an inch in length. On agitating the contents of the phial, they were eafily detached from the furface of the charcoal, to which they feemed to adhere but very flightly.

Thefe experiments were repeated feveral times, and always with precifely the fame refults.

When the oxide of gold was reduced in this way, the revived metal appeared under the form of fmall fcales, adhering firmly to the furface of the charcoal. May not the difference of

of the forms under which gold and ſilver are revived from their oxides, in this proceſs, be owing to the difference of the ſpecific gravities of thoſe metals?

The following experiments, which were firſt ſuggeſted by an accident, were made with a view to inveſtigate ſtill farther the cauſes of thoſe effects which have been attributed to the ſuppoſed chemical properties of light.

Having accidentally put away two ſmall phials, each containing a quantity of aqueous ſolution of the oxide of gold and ſulphuric ether, in each of which the ether had extracted the gold completely from the ſolution, as was evident by the yellow colour of the ſolution having been transferred to the ether, and the ſolution being left colourleſs; in one of the phials, which happened to ſtand in a window in which there was occaſionally a ſtrong light (though the direct rays of the ſun never fell on it), I found, in about three weeks, that the oxide was almoſt entirely reduced; the revived gold appearing in all its metallic ſplendour in the form of a thin pellicle, ſwimming on the ſurface of the aqueous liquor in the phial, and the colour of the ether which repoſed on it having become quite faint; while no viſible change had been produced in the contents of the other phial, which had ſtood in a dark corner of the room.

As theſe appearances induced me to ſuſpect, or rather ſtrengthened the ſuſpicions I had before conceived, that the ſeparation of gold from ether, under its metallic form, when a ſolution of its oxide is mixed with that fluid, is always effected by a reduction of the oxide by means of light, I made the following experiment, with a view to the farther inveſtigation of that matter.

Experiment No. 13. Into a ſmall pear-like phial of very fine tranſparent glaſs I put equal quantities of an aqueous ſolution of the muriatic oxide of gold and ſulphuric ether; and the phial, which was about half filled, being cloſed with a good cork well ſecured in its place, was expoſed to the action of the direct rays of a bright ſun.

A pellicle of revived gold, in all its metallic ſplendour, began almoſt immediately to be formed on the ſurface of the aqueous liquid, and ſoon covered it entirely; and at the end of two hours the whole of the oxide was completely reduced, as was evident from the appearance of the ether, which became *perfectly colourleſs.*

On ſhaking the phial, the metallic pellicle, which covered the ſurface of the aqueous liquid, was broken into ſmall pieces, which had exactly the appearance of leaf-gold, poſſeſſing the true colour and all the metallic brilliancy of that metal.

On ſuffering the phial to ſtand quiet, the aqueous liquor and the ether ſeparated, and moſt of the broken pieces of the thin ſheet of gold deſcended to the bottom of the phial: the remainder of them floated on the ſurface of the aqueous liquid; and the ether, as well as the aqueous liquid, appeared to be perfectly tranſparent and *colourleſs.*

By the length of time which was required for the ether and the aqueous liquid to ſeparate, I thought I could perceive that the ether had loſt ſomething of its fluidity; but as this was an event I expected, it is the more likely, on that account, that I was deceived, when I imagined I ſaw proofs of its having taken place.

On removing the cork, after the contents of the bottle had been ſuffered to cool, there was no appearance of any conſiderable quantity of air, or other permanently elaſtic fluid, having been either generated or abſorbed during the experiment.

Finding that the oxide of gold might be ſo completely and ſo expeditiouſly reduced by means

means of ether, I conceived it might be poffible to perform that chemical procefs, *in the moift way*, by means of effential oils; and this conjecture proved to be well founded.

Experiment No. 14. Upon a quantity of a diluted aqueous folution of nitro-muriate of gold, in a fmall pear-like phial, about 1½ inch in diameter at its bulb, was poured a fmall quantity of etherial oil of turpentine, juft as much as was fufficient to cover the aqueous folution to the height of ²⁄₀ of an inch; and the phial, being well clofed with a good cork, well fecured, was expofed one hour to the heat of boiling water in a fteam-veffel.

The gold was revived, appearing in the form of a fplendid pellicle, of a bright gold colour, which floated on the furface of the aqueous liquid. The oil of turpentine, which, at the beginning of the experiment, was as pale and colourlefs as pure water, had taken a bright yellow hue; and the aqueous fluid, on which it repofed, had entirely loft its yellow colour.

On fhaking the phial, its contents were intimately mixed; but, on fuffering it to ftand quiet, the oil of turpentine foon feparated from the aqueous liquid, retaining its bright yellow hue, and leaving the aqueous liquid colourlefs.

On fhaking the phial, *before it had been expofed to the heat*, and mixing its contents, and then fuffering it to ftand quiet, the oil of turpentine, on taking its place at the top of the aqueous folution, was not found to have acquired any colour; nor was the bright gold colour of the folution found to be at all impaired. When fulphuric ether was ufed inftead of the oil of turpentine, the effect was in this refpect very different.

To find out whether the oil of turpentine ufed in this experiment, and which had acquired a deep yellow colour, had loft that property by which it effected the reduction of the metallic oxide, I now poured an additional quantity of the aqueous folution of the oxide into the phial, and, fhaking the phial, expofed it with its contents to the heat of boiling water.

After it had been expofed to this heat about two hours I examined it, and found, that though a confiderable quantity of gold had been revived, yet the aqueous liquid ftill retained a faint yellow colour.

The oil of turpentine had acquired a deeper and richer gold colour, approaching to orange.

To the contents of the phial I now added about half as much diftilled water, and, mixing the whole by fhaking, I expofed the phial again, during two hours, to the heat of boiling water; when the remainder of the oxide was reduced, and the aqueous liquid left perfectly *colourlefs*.

On repeating this experiment with oil of turpentine, and varying it, by ufing a folution of the oxide of *filver* (an aqueous folution of nitrate of filver,) inftead of that of *gold*, the refult was nearly the fame: the metal was revived, and the oil of turpentine acquired a faint greenifh-yellow colour.

I alfo revived the oxides of gold and of filver with *oil of olives*, by a fimilar procefs, with the heat of boiling water. The oil of olives ufed in thefe experiments loft its tranfparency, and became deeply coloured: that ufed in the reduction of the oxide of filver taking a very deep dirty brown colour, approaching to black; and that employed in reducing the oxide of gold being changed to a yellowifh-brown, with a purple hue.

In

In the experiment with the oxide of ſilver, the inſide of the phial, in the region where the oil repoſed on the aqueous ſolution, was beautifully ſilvered, the revived metal forming a narrow metallic ring, extending quite round the phial; and, in both experiments, ſmall detached pellicles of revived metal were viſible in the oil, and adhered in ſeveral places to the inſide of the phial, forming bright ſpots, in which the colour of the metal and its peculiar ſplendour were perfectly conſpicuous.

Experiment No. 15. As *carbon* is one of the conſtituent principles of ſpirit of wine, as well as of eſſential oils and ſulphuric ether, I thought it poſſible that I might ſucceed in the reduction of the oxide of gold, by mixing alcohol with an aqueous ſolution of nitro-muriate of gold, and expoſing the mixture in a phial well cloſed to the heat of boiling water; but the experiment did not ſucceed.

By pouring upon this mixture a ſmall quantity of oil of olives, and expoſing it again to the heat of boiling water, the gold was revived.

Is it not probable that the reaſon why the oxide was not reduced by alcohol, is the mobility of thoſe elements, which ought to act on each other, in order that the effect in queſtion may be produced? I have no doubt but the oxide would be reduced, could the alcohol be made to reſt on the ſurface of the aqueous ſolution without mixing with it.

I wiſhed to have been able to have collected and examined the elaſtic fluids, which probably were formed in moſt of the preceding experiments; but my time was ſo much taken up with other matters that I had not leiſure to purſue theſe inveſtigations farther.

In order to ſee what effects would be produced by the heat generated at the ſurface of an opaque body, of a nature different from thoſe hitherto uſed in the reduction of the metallic oxides, and one that is little diſpoſed to form a chemical union with oxygen, (*magneſia alba*) when, being immerſed in an aqueous ſolution of the oxide of gold, the rays of the ſun were made to impinge on it, I contrived the following experiment.

Experiment No. 16. I took four ſmall thin phials, A, B, C, and D, of very fine glaſs, and putting into each of them about five grains of dry *magneſia alba*, I filled the phial A, nearly full, with a ſaturated aqueous ſolution of the oxide of gold.

I filled the phial B, in like manner, with ſome of the ſame ſolution, diluted with an equal quantity of diſtilled water; and the phials C and D were filled with the ſolution ſtill farther diluted.

Theſe phials, open or without ſtoppers, were expoſed one whole day to the action of the direct rays of a bright ſun, their contents being often well mixed together during that time by ſhaking.

The contents of all theſe phials changed colour more or leſs, but they acquired very different hues. The contents of the phial A became of a very deep rich *gold colour*, approaching to *orange*, the earthy ſediment being throughout of the ſame tint.

The contents of the phial B, which were at firſt of a light ſtraw colour, firſt changed to a light green, and then to a greeniſh blue. The phial having been ſuffered to ſtand quiet ſeveral days, in an uninhabited room in a retired part of the houſe, the ſolution became nearly colourleſs, and the ſediment was found to be of a dirty olive colour.

The

The colour of the contents of the phials C and D was changed nearly in the same manner; and having been suffered to stand quiet two or three days to settle, the solution was found to be quite colourless, and the sediment to be deeply coloured. There was, however, a very remarkable difference in the hues of the two phials; that of the phial C being of a light greenish-blue; while that in the phial D was indigo, and of so deep a tint that it might easily have been taken for black.

These appearances were certainly very striking, and well calculated to excite my curiosity; but I am so much engaged in public business that it is not at present in my power to pursue these inquiries farther. I wish that what I have done may induce others, who have more time to spare, to devote some portion of their leisure to these interesting investigations.

VII.

Some Account of the Persian Cotton-Tree. By MATTHEW GUTHRIE, *M.D. F.R.S. &c. &c.**

COTTON is a plant of both the old and the new world; at least it is found wild in both: but I have my doubts whether it was a native of America before the Europeans carried it over, and shall assign reasons for my incredulity when I come to treat of the *Persian cotton*, which is the very species that is said to be American.

Five species of the cotton-tree are enumerated by Linnæus; and there is reason to suspect the existence of a sixth, if what we are told of the extreme fineness and silky nature of a particular kind reared in some of the Antilles be literally true. This curious variety is called Siam cotton, because the reed was originally obtained from Siam.

The first species of cotton is the *gossypium arboreum*, or *Indian cotton-tree*, which has been cultivated and manufactured in the East Indies from the remotest period of the authentic history of that country, or between three and four thousand years. It delights in a sandy soil.

The second species is the *gossypium religiosum*, which is likewise a native of India, and a tree, or at least a high shrub; but why Linnæus dignified it with so singular a specific name I shall leave the learned Asiatic Society in Bengal to determine, as they must know if it be used for any religious purposes by the Bramins. This species of cotton is said to be that which is cultivated by the French in Martinico.

The third is the *G. barbadense*, a species of biennial cotton shrub cultivated in our British island of Barbadoes, from which it obtains its specific name. I believe it is likewise the same species which is cultivated in Jamaica.

The fourth is the *G. hirsutum*, an American *perennial* cotton shrub in the warmer provinces, but *annual* in the colder, as is sometimes the case with plants in climates where their roots lose their vegetating power by winter frost.

The fifth and last species is the *G. herbaceum*, or *G. annuum*, an annual cotton plant which rises to the height of three or four feet, and is sown and reaped, like corn, twice a year

* Manchester Memoirs, Vol. V. Part I.

year in hot countries, and once a year in colder climates. It bears a large yellow flower with a purple centre, and fruit about the ſize of a walnut containing the cotton.

This is the famous *Perſian cotton* properly the ſubject of the paper, although a ſlight mention of other ſpecies was neceſſary to give a more complete view of the ſubject. Linnæus calls it a native of America; and there is no doubt but that it is become ſo, although there is much more reaſon to ſuppoſe America naturalized a Perſian plant than that Perſia got it from the new world; eſpecially if we are to credit a paper lately preſented by a Britiſh merchant to the Economical Society at Peterſburgh, in which it is poſitively aſſerted that ſeveral of the European nations furniſhed their American colonies with *Perſian cotton* ſeed procured at Smyrna. Now this fact (if ſufficiently authentic, which I do not doubt from my knowledge of the veracity of the author) will eaſily account for the *G. herbaceum* being found wild in America; when we recollect the wonderful proviſion of nature for the wide diſperſion of ſeeds, and Linnæus's aſſertion that the *Erigeron canadenſe* was diſperſed from the botanic garden of Paris by the winds over a great part of Europe, and ſeveral other plants* from the botanic garden of Upſal over a whole province.

My reaſons for ſuggeſting theſe doubts relative to the native country of this ſpecies of cotton are, that all vegetables of this genus are ſuppoſed to have been indigenous in Perſia excluſively, and that even the Eaſt Indies derived the cotton plants from thence; a conjecture which ſeems to have acquired ſome degree of credit from the late diſcovery of Sir William Jones, viz. that the Hindoos, or inhabitants of India, were originally a colony of the ancient Iran or Perſia, which ſeems to have been the cradle of the human ſpecies, ſince its ancient language appears to have been the mother of all thoſe now exiſting (with the exception of the Arabic and Tartarian), of which neverthleſs it contained many words.

Now it is very poſſible that the firſt colony carried the cotton plant with them to India, and that it was afterwards diſperſed from Hindoſtan to the adjacent countries and iſlands. The cotton plant is widely diſperſed likewiſe throughout Europe and ſome parts of Africa, particularly the annual or herbaceous ſpecies (the very plant treated of here) reared in the north of Perſia, and which is alſo cultivated in Malta †, Sicily, Chio, Lemnos, and other iſlands of the Archipelago, although poſſibly the cotton of theſe iſlands may be varieties of the ſpecies from difference of ſoil, climate, &c.

The beſt of the European cotton is brought from Cyprus; but Smyrna, Aleppo, Damaſcus, Jeruſalem, &c. furniſh likewiſe a quantity of cotton at leaſt equal to the European.

Cultivation of Cotton in Perſia.

THE annual cotton, or this laſt ſpecies, of which we have treated more amply, is much cultivated in the northern or colder provinces of Perſia bordering on the Caſpian Sea (as the perennial is in the ſouthern); and it is from thence that the ſeeds now ſent to Portugal have been obtained through the Bucharian Tartar merchants, and are the production of the *Goſſypium herbaceum* of Linnæus, the *Goſſypium annuum* of Pallas. It is ſown in Perſia from the end of March to the end of April, and reaped in September. This ſpecies requires a rich ſoil mixed with ſand; and therefore where the land is not rich enough they

* The Antirrhinum minus, the Datura ſtrantonium, the Gnaphalium americanum, &c.

† There is a kind of cotton cultivated in Malta, of a nankeen colour, which exceeds in fineneſs all other cotton, and is much ſuperior even to that from the Antilles.

manure

manure it with cow or sheep dung; although we are told that when the plants are once raised above the ground any species of soil will answer. The ground is worked in the spring, and the seeds are planted at the distance of eight or ten inches from one another, whilst care is taken to weed it, to give air to the young plants. Dry summers give the best crop, as rain is more particularly hurtful when it falls in great quantities during the flowering and ripening of the cotton. It is gathered, as said above, in September, care being always taken to collect a sufficient quantity of seed for the next year. Lastly, watering the young plants with a mixture of wood-ashes and water in certain situations is sometimes necessary to guard them from destructive worms.

The Russians have cultivated the same species of Persian cotton in the government of Caucasus, and rear enough of it to serve their own national manufactures, which are not as yet either numerous or considerable; but on the Terck, at the foot of the Caucasus, where it is reared, they do not sow till the middle of May, lest a late spring frost, which is sometimes felt in those parts, should destroy the hopes of the planter. With that one exception, the Russians strictly observe the Persian mode of cultivation.

There is a species of silky cotton much cultivated at present in Germany, which possibly may merit the attention of Portugal for their plantations in America. It is the *Asclepias syriaca* of Linnæus, and affords so fine a species of cotton (if I may so name it) that fabrics have been erected in Saxony, where stuffs are made of it which rival in lustre, &c. the true animal silk. But this new vegetable silk has circumstances attending it that seem to recommend its cultivation in some of the American colonies and islands: First, because it is originally the native of a hot climate, as Linnæus's specific name indicates; and of course it is likely to be in its greatest beauty and excellence in climates which approach nearest to that of its native country. Secondly, because its stalks afford a coarse sort of cloth well calculated to clothe negroes, whilst from the pith of them paper is made.

VIII.

Facts and Observations concerning the Measure and Expence of first Movers, namely, Wind, Water, Steam, and Animal Strength, and on other Objects of general Utility. (*W. N.*)

THE consideration of the value and importance of natural first movers is of consequence not only to practical engineers, but to every individual in cultivated society. There are numberless situations, even in the spirited manufacturing kingdom we inhabit, where large revenues are expended to perform mechanical and hydraulic operations by the force of human labour, or by horses, which might be for the most part saved by substituting a steam-engine or windmill, or making use of a stream of water now running to waste. It is well known, that, since the extension of the cotton and other works, estates of small rent in the neighbourhood of Manchester and elsewhere have been, and continue to be, let at more than twenty times their original rent, merely from the fortunate circumstance of their possessing a small stream of water falling with a sufficient declivity to give motion to a mill. If the proprietors of lands and manufacturers in general were better acquainted with the simple methods of estimating the forces of those currents of water which run neglected through

through their grounds and premifes, and which an intelligent obferver need not walk many miles in any country to point out, their property and revenue might immediately receive a confiderable acceffion; and the community would be ftill more effectually benefited. The inceffant demands for the employment of fuch forces in grinding corn, colours, drugs, tobacco; in cutting bark and other tanners' and dyers' materials; in fawing wood; in laminating, drawing, or fafhioning metallic bodies; in fpinning, weaving, fulling, &c. the products of the organized kingdoms by arts already practifed, exclufive of the many improvements which may be expected in their application, are too numerous to afford the leaft reafon for any proprietor to fear a want of employment, or to confider the erection of a mill in a proper fituation as a fpeculation of the leaft danger or probable difadvantage. Similar obfervations are to a certain extent applicable in favour of the ufe of horfes inftead of men, and fteam inftead of horfes, in every cafe where the power is required to be great or long continued, and the fkill either little, or capable of being fupplied by machinery.

I am tempted to digrefs for a moment from my fubject by the natural recurrence of a political reflection, fo obvious that it fcarcely ever fails to be made when the extenfion of machinery and the application of inanimate powers are confidered. It is ftated by certain humane but miftaken objectors, that the fcheme of mechanical and chemical improvement is pointed againft the human fpecies, and tends to drive them out of the fyftem of beneficial employment. Two creatures offer themfelves to me for employment and fupport—a man and a horfe. I muft invariably prefer the latter, and leave the former to ftarve. Two other beings—a horfe and a fteam-engine, are candidates for my favour. My preference to the latter tends to exterminate the fpecies of the former. In both cafes it is ftated, that the number of intelligent creatures capable of the enjoyment of happinefs muft be diminifhed for want of fupport; and that, on the whole, the fum of the propofed improvements is not only a lefs proportion of good to fociety, but a pofitive acceffion of much mifery to the unemployed poor.

On this wide and extended argument, which can in fact be maintained againft improvements in no other way than by infifting that the favage ftate, with all its wants, its ignorance, its ferocity, and its privations, is preferable to the focial intercourfe of effort and divifion of labour we are habituated to prefer, it may be fufficient to obferve, that the topic includes matter not only for reafoning and induction, but alfo for experiment. By reference to the matter of fact, though it muft be allowed that new improvements, which change the habits of the poor, muft at firft expofe them to temporary inconvenience and diftrefs, againft which, in fairnefs, it is the duty of fociety to defend them; yet the invariable refult of fuch improvements is to better the condition of mankind. The nations which have fhewn the moft ingenuity and induftry in this way are not only the richeft, the moft populous, the moft intelligent, and the beft defended; but the provinces of thofe nations are feen to flourifh in proportion to their refpective degrees of activity in this refpect. And from thefe exertions it is, as Smith* emphatically remarks, that "the accommodation of an European prince does not always fo much exceed that of an induftrious and frugal peafant, as the accommodation of the latter exceeds that of many an African king, the abfolute mafter of the lives and liberties of ten thoufand naked favages."

* Wealth of Nations, i. ch. i.

But

But to return to our ſubject. In this practical and popular communication I ſhall avoid entering into any diſcuſſion of the theory of windmills, which has employed the attention of ſo many eminent men for a century paſt. The advantages of theſe engines for ſuch work as will admit of being performed and ſuſpended by intervals are ſufficiently known, but perhaps not ſufficiently attended to. There can be no doubt but that the Dutch, who uſe windmills for ſawing, pumping, and various other works, as well as grinding, muſt have found them very profitable, ſince their country is overſpread with them. It may be proper however to take notice, that many writers have copied one from another the determination of Maclaurin, that the beſt angle for windmill-ſails to make with the line of direction of the wind is 54° 44′, which is only true, as that excellent mathematician obſerves, at the very commencement of the motion, and requires to be enlarged as the velocity of the ſails increaſes: for the law of which, and other eſſential objects, his account of Sir Iſaac Newton's Philoſophical Diſcoveries, and his Treatiſe on Fluxions, may be conſulted. Smeaton, who had much experience in the buſineſs of a civil engineer, and whoſe data may always be depended on, though his theories are not conſtantly accurate, made a ſet of experiments on the conſtruction and effects of windmill-ſails, which are deſcribed in the Philoſophical Tranſactions for the year 1759*. This engineer ſtates, that the mean rate of work for mills with Dutch ſails is when they make about thirteen turns in a minute, which is when the velocity of the wind is $8\frac{2}{3}$ miles in an hour, or $12\frac{2}{3}$ feet in a ſecond: and this wind in common phraſe would be called a freſh gale. Taking the maximum of Deſaguliers, hereafter to be mentioned, as his ſtandard for computation, he deduces the ſize of a windmill-ſail of the figure juſt mentioned, and alſo according to a figure conſtructed from his own experiments, which ſhall be equal in mean power to one man; and thence he arrives at the inference, that one of his own ſails, thirty feet in length, will, when working at a mean rate, be equal to the power of 18,3 men. He had an opportunity of verifying this in the large way in a mill uſed for cruſhing rape-ſeed. The mean power of a windmill is therefore very conſiderable; but what may be the annual or average quantity of work ſuch an apparatus is capable of performing under all the viciſſitudes of the wind, I poſſeſs no means of aſcertaining.

In the ſame treatiſe Smeaton makes ſeveral very juſt remarks on thoſe windmills which are acted upon by the direct impulſe of the wind againſt ſails fixed to a vertical ſhaft. His objections have, I believe, in every inſtance been juſtified by the inferior efficacy of theſe mills when compared with the charges of erecting them. He alſo maintains that water-mills with oblique ſails, upon the principle of the common windmill, cannot prove beneficial to the undertaker. It is indeed probable that moſt of the circumſtances of running-water are likely to render the common over and underſhot-wheels cheaper and more effectual, and that the oblique float-board will in no inſtance come near the effect of a cloſe overſhot wheel. But it is alſo certain that ſuch wheels are uſed in China, in the ſouth of France, and elſewhere, with much more effect than Smeaton appears diſpoſed to think them capable of: and at all events, the ſubject of theſe wheels deſerves to be conſidered.

* This account has ſince been republiſhed, together with other valuable papers of the ſame author, under the title of An Experimental Inquiry concerning the Natural Powers of Wind and Water to turn Mills, &c. By John Smeaton, F. R. S. 8vo, printed for Taylor, London, 1794.

 The

The enquiry which moſt immediately intereſts land-holders, and others who have the advantage of a current of water, is to aſcertain whether it will afford ſufficient power to juſtify the erection of a mill, and what that power may be. If the ſtream be ample without much fall, it muſt neceſſarily be applied to move an underſhot wheel by its impulſe, and the power will be determinable from the velocity of the water, and the quantity which paſſes through the ſection of its bed. One of the eaſieſt methods of aſcertaining theſe data is that given by Deſaguliers *, as follows. Obſerve a place where the banks of the river are ſteep and parallel, ſo as to make a kind of trough for the water to run through, and by taking the depth acroſs make a true ſection of the river. Stretch a ſtring at right angles over it, and at a ſmall diſtance another parallel to the firſt. Then take an apple, an orange, or other ſmall ball, juſt ſo much lighter than water as to ſwim in it, and throw it into the water above the ſtrings. Obſerve when it comes under the firſt ſtring by means of an half-ſecond pendulum, a ſtop-watch, or any other proper inſtrument, and likewiſe when it arrives at the ſecond ſtring. By this means the velocity of the upper ſurface, which in practice may generally be taken for that of the whole, will be obtained. The ſection of the river at the ſecond ſtring muſt be aſcertained by taking the depth as before. If this ſurface or ſection be the ſame as the former, it may be taken for the mean ſection ; if not, add both together, and take half the ſum for the mean ſection. The area of the mean ſection in ſquare feet being then multiplied by the diſtance between the ſtrings in feet, will give the contents of the water in ſolid feet, which paſſed from one ſtring to the other during the time of obſervation. And this, by the rule of three, may be adapted to any other portion of time. Suppoſe, for example, the time had been 12″, and the hourly expenditure of water were required, the proportion would be : As 12″ are to 3600″, ſo is the number of cubic feet obſerved to the hourly expenditure in cubic feet. If the mere velocity be required in proportion to any fixed interval of time, the ſame proportion will give it, provided, inſtead of the ſolid contents in the third term, there be taken the diſtance between ſtring and ſtring.

The intelligent obſerver may in general abridge this operation, by taking notice of the arrival of the floating body oppoſite two ſtations on the ſhore, eſpecially when it is not convenient to ſtretch a ſtring acroſs. The arch of a bridge is a good ſtation for an experiment of this kind, becauſe it affords a very regular ſection and two fixed points of obſervation : and in ſome inſtances the ſea practice of heaving the log may have its advantages. Where a time-piece is not at hand it may be equally convenient, provided two obſervers attend, to note the time with a half or quarter-ſeconds pendulum. The half-ſeconds pendulum is made by ſuſpending a ſmall round button, or other ſpherical weight, by a thread looped over a pin of ſuch a length that the diſtance from the bend of the loop to the centre of the weight ſhall be 9,8 inches. The quarter-ſeconds pendulum is one fourth of this length. If, by obſervations at ſeveral ſtations above and below any particular point of the river, the velocity is not found to vary, the ſection of the river in all that ſpace may be concluded to be uniform ; and it will not be neceſſary to determine more than one ſection by actual meaſurement.

In the caſe of an overflowing pond, or ſmall ſtream, which will admit of a dam acroſs it,

* Courſe of Experimental Philoſophy, ii. 419.

the

the quantity of water afforded may be afcertained by fuffering it to run through a notch in a board, or a vertical hole of an inch fquare, according to the following table, which Defaguliers fays he calculated from repeated experiments*.

A TABLE of the Expence of Water through an Inch-fquare Hole, and through a Cut an Inch wide and of different Depths.

Inches below the furface.	Expence of water through a hole of a fquare inch at different depths, according to the number of inches in the firft column. Tuns in an Hour.	Expence of water through a notch of different depths, according to the number of inches in the firft column.
1	1,04	1,04
2	1,46	2,50
3	1,79	4,29
4	2,08	6,37
5	2,31	8,68
6	2,53	11,21
7	2,74	13,95
8	2,92	16,87
9	3,12	19,19
10	3,28	22,47
11	3,44	25,81
12	3,58	29,39
13	3,74	33,13
14	3,88	36,91
15	3,95	40,86
16	4,16	45,02
17	4,28	49,30
18	4,40	53,70
19	4,52	58,22
20	4,62	62,84
21	4,76	67,60
22	4,87	72,47
23	4,94	77,41
24	5,06	82,47
25	5,2, or 5 tuns and 50,4 gallons.	87,67

A cubic foot of water weighs very nearly $62\frac{1}{2}$ pounds averdupois, and an hogfhead of water weighs about 550 pounds.

* Courfe of Experimental Philofophy, ii. 129.

In the conſideration of power or force to be derived from water in motion, the water may be taken as a determinate maſs falling through a given height in a given time. In order that this deſcending weight may cauſe another weight to aſcend, or may overcome ſome reſiſtance in the way of work with that degree of ſpeed which ſhall be the moſt profitable, it is neceſſary that the reſiſtance or work to be done ſhould be neither too great nor too little. If it be too great, the ſlowneſs of operation will diminiſh the quantity of work; and if it be too ſmall, the ſpeed will not ſufficiently compenſate for this ſmallneſs. When the power is therefore known, it remains to deduce what may be the effect. But in the firſt place, as the height from which the water flowing in a river may have deſcended, in order to acquire its velocity, is, from a variety of circumſtances, difficult to be aſcertained, and alſo very different from that height which would immediately and without impediment produce the ſame velocity, it becomes neceſſary to compute this laſt height, which hydroſtatical writers uſually call the height of the virtual head. Theſe writers teach, that the velocity of a fluid ſpouting through an orifice in a thin plate is the ſame as would be acquired by a body falling in clear ſpace from the height of the ſurface of the fluid above the orifice. Hence, from the common doctrine of falling bodies, if the uniform velocity of a ſtream be expreſſed in feet per ſecond, the virtual height of the fall will be found by multiplying the given velocity into itſelf, and dividing the product by 64,2882; the quotient will be the required height expreſſed in feet *.

The effect of underſhot and overſhot-wheels has been treated by various authors, who have given reſults extremely different from each other. Smeaton, in the Treatiſe often quoted in the courſe of this communication, obſerves, that Belidor in his Architecture Hydraulique, i. 286, endeavours to demonſtrate that water applied underſhot will do ſix times more execution than the ſame applied overſhot; while Deſaguliers, whom he (Smeaton) miſquotes by overlooking the difference of fall, is ſaid to have given the advantage as ten to one in favour of the overſhot †. The particular experiments of Smeaton himſelf, as well as his experience, point out the following reſults.

The effect in underſhot-mills in the large way is at beſt one third of the power; that is to ſay, the wheel, being driven with two-fifths of the velocity of the ſtream, will raiſe a quantity of water equal to one-third of the column, which ſtrikes the float-boards, to an height equal to that of the virtual head or fall: and the effect of an overſhot-wheel will be, at a medium, twice that of the underſhot. Mills having a breaſt-wheel, or other kind of wheel on which the water acts, partly by its weight and partly its impulſe, will produce more or leſs effect, accordingly as the circumſtances approach more nearly to thoſe of the over or underſhot-wheels.

For the advantage of ſuch as are leaſt converſant in ſubjects of this nature, for whom chiefly the preſent memoir is intended, I ſhall illuſtrate the ſubject by an example.

Suppoſe a ſtream to paſs through an eſtate without any evident fall, with a velocity of nine feet per ſecond, and affording ſufficient room to place an underſhot-wheel with a pro-

* The proof of this is ſimple, but may alſo be ſeen in Deſaguliers, ii. 520.—Thoſe who uſe logarithms may with leſs trouble *ſubtract the conſtant log.* 1.8081312 *from twice the log. of the velocity, and the remainder will be the log. of the virtual height.*

† The proportion of work of the two actual mills, compared by Deſaguliers, is as 3,25 to 1 in favour of the overſhot.

per

per number of float-boards, each six feet long and two deep: It is required to determine, 1st, What quantity of water might be raised by that power, during the day of ten hours, to the height of thirty feet? or, 2dly, What number of bushels of wheat, or malt, might be ground in the same time? or, 3dly, What number of men or horses might this wheel supply the place of, in performing any other kind of work or manufacture?

In the first place, to find the virtual fall, multiply 9 feet by itself, which gives 81, and divide this by 64,2882: the quotient will be 1,26 feet.—Again, the surface of the float being 6 feet by 2 feet, is 12 square feet; which, multiplied by the velocity 9, gives 108 = the number of solid feet of water which has in effect fallen from the virtual head in a second. But this power will partly be consumed in producing eddies and lateral motions, and partly remain uncommunicated in the tail water as it flows off. For which reasons, as has already been remarked, the effect will only be one-third of the power. That is to say, one-third part of 108, or 36 cubical feet of water will be raised in a second to the height of 1,26 feet. The question however is directed to the height of thirty feet, and the quantities of water, or other weights, raised by equal powers, are inversely as their heights: that is, 30 feet : 1,26 feet :: 36 cubical feet : 1,512 cubical feet raised 30 feet in a second. But the hour consists of 3600 seconds, and the day of ten hours, = 36000, which multiplied by 1,512 produces 54432 cubic feet, or 6437 hogsheads, (reckoning the gallon at 231 solid inches) which are raised 30 feet in the day *.

With regard to the operation of grinding, it is allowed that about 3420 cubic feet of water with a fall of 10 feet will, by an overshot wheel, grind one bushel of wheat into flour, and the same force will cut five quarters of malt. But our undershot wheel will only do half the work. From the foregoing calculations our stream is equivalent to 13,6 solid feet per second with a fall of ten feet, or 816,5 solid feet per minute. Therefore as 3420 cubic feet : one bushel of wheat :: 816,5 cubic feet : 0,239 of a bushel which would be ground per minute by an overshot wheel; or 0,1195 by our undershot wheel. This last number multiplied by 60 produces 71,7 bushels per hour, or nearly 72 bushels, or 9 quarters of wheat per day of ten hours. And by the proportion of malt to wheat just mentioned, it will follow that 360 quarters of the former grain might be cut in the same time.

Animal strength is of so fluctuating a nature, that it is difficult to subject it to any estimate. Physical causes must affect both the quantity and duration of the efforts

* This is an outside estimate, and gives no allowance for frictions and errors in the construction of the hydraulic work. The reaction in Smeaton's Experiments was afforded merely by a weight and pulley, which is greatly in favour of the mill in deducing the effect. I find in the "Reports of the late Mr. John Smeaton, F. R. S." printed in quarto, London 1797, the power of the water called "Hubbert's mill stream" is deduced from the quantity and fall, (p. 245.) by a rule which he does not mention, but is evidently this, for overshot or close breast wheels: *Multiply the number of cubic feet of water discharged per minute by the fall in feet: this number may be called the power. Divide the power by two, and the quotient may be called the effect. Assume any height at pleasure in feet: divide the effect by the height, and the quotient will be the cubic feet of water, which a good hydraulic apparatus will raise to that height per minute.*

The undershot wheel will raise half that quantity: or in general less than one-sixth of the power.

I have retained the proportion of the Treatise on mills in the text, because the ratio of the effect to the power can be easily altered at pleasure in computing.

possible

possible to be made either by man or beast, and the former is more particularly influenced by his moral habits. From these last it is that the influence of reward, the expectation of favour or patronage, and various other similar motives, have operated in the temporary exhibition of hydraulic machines, to produce results contrary to every sound deduction from permanent work, and most perniciously delusive to the parties concerned in supporting or encouraging such engines. Desaguliers*, who has taken much pains to ascertain the maximum of power in this respect, has determined that a man can raise of water, or any other weight, about 550 lbs. or one hogshead ten feet high in a minute; and he states that a horse will raise five times that quantity; or, which is the same thing, that quantity through five times the height. His deduction respecting the man, though he says it will hold good for six hours, appears from his own facts to be too high, and certainly such as could not be maintained one day after another. Smeaton † considers this work as the effort of haste or distress. He reports ‡ that six good English labourers will be required to raise 21141 solid feet of sea water to the height of four feet in four hours. This quantity is of the same weight as 21669 cubic feet of fresh water, with which, and the above rate, it will be found by an easy calculation that the men will raise a very little more than six cubic feet each to the height of ten feet in a minute. But the hogshead containing 8½ cubic feet, Smeaton's allowance of work proves less than that of Desaguliers in the proportion of 6 to 8½. And as his good English labourers, who can work at this rate, are by him estimated to be equal to a double set of common men picked up at random; it seems very proper to state that, with the probabilities of voluntary interruption, and other incidents, a man's work for many days together ought not to be estimated at more than half a hogshead raised ten feet high in a minute. In the same report last quoted, Smeaton states, that two ordinary horses will do the work in three hours and twenty minutes, which amounts to a little more than two hogsheads and a half § raised ten feet high in a minute. One horse will therefore do the work of five men.

To apply these deductions to our example, it must be recollected that the quantity raised in ten hours to the height of thirty feet was inferred to be 6437 hogsheads, which are equivalent to 32 hogsheads raised ten feet high per minute. Consequently at the rate of one man for each half hogshead, the stream would perform the work of 64 men, or nearly thirteen horses.

With regard to streams which fall by a considerable declivity, the water may be conveyed by the well known means of a dam or trough to the buckets of an overshot wheel, placed in that part of the stream which is found the most convenient in point of expence and local situation. Suppose, for example, the fall amounted upon the whole to fifteen feet, upon a length of 200 yards, it might be more convenient to lead the whole stream in a wooden trough supported upon posts, with a slight declivity, to the wheel near the lower end of the current; or in other circumstances, according to the face of the land, the work might prove cheaper or more durable if the wheel were placed near the upper end of the stream, and the channel sunk so as to convey away the tail water with no more fall than should be necessary for that purpose. In this case it is obvious that the fall must be ascer-

* Course of Lectures, ii. 498, 505, 536. † Reports, i. 216. ‡ Ibid. i. 323.

§ He elsewhere (ibid. p. 229.) rates an horse at 250 hogsheads, ten feet high, in an hour; but I prefer the deduction in the text.

tained by the operation of levelling, and not by computation as in our former supposition. This operation is easy, and may be performed upon such short distances with sufficient accuracy, with a carpenter's square and a staff.

For this purpose it will be necessary to drive a pin into the handle, or thickest part of the square, near the corner, upon the flat side. A line must be drawn from the pin, parallel to the edge of this handle, and a looped thread supporting a small weight must be hung upon the pin. In this situation the instrument is ready for taking levels. For if it be held in such a position that the plumb line may cover the line drawn from the pin, the blade will lie horizontal, and by looking along its upper edge the eye will be directed to some object on the same level with itself. Let the observer provide himself with a staff five or six feet long; which, if divided into feet and inches, will be the more useful. This is to be pitched at or near the lower end of the stream, and against it the observer is to press the handle of his square, which may be conveniently done by grasping both it and the staff together in the same hand. Then, holding the apparatus so that the plumb line may lie fair, he is to direct the blade of the square to some stone or other remarkable object higher up the stream; for which purpose it will be necessary to slide the handle of the square up or down the staff till the positions are accurately obtained; that is to say, till the blade points fairly to the object at the same time that the plumb line denotes that the blade is level. When this is done, a memorandum must be taken of the feet and inches from the bottom of the staff to the upper edge of the blade, and the observer must proceed to the stone or object to which his sight was directed, and, planting his staff there, repeat the same operation with regard to some other object still higher up the stream, and again note down the feet and inches observed upon the staff. This process continued will, by a few reiterations, bring him to the upper extreme of the water which we suppose to be within his grounds or power. The sum of all the measures taken upon the staff is the difference of level or whole fall of the water. The quantity of water in cubic feet may be ascertained by either of the means before mentioned; or, if the stream be very small, it may actually be dammed, and measured off with two tubs or measures, of as large a size as can conveniently be managed.

I should be inclined to apologize to such of my readers as are intimately acquainted with these subjects, for the minuteness of detail into which I have entered, if I were not assured that they will be the first to perceive, that many individuals who possess beneficial streams are totally uninformed of their value, or of the means of determining the same.

(To be continued.)

IX.

Enquiries concerning the Invention and Practice of the Art of Hat making.

To Mr. NICHOLSON.

Newcastle, 15th Dec. 1798.

SIR,

YOUR Publication being open to enquiries relative to the advancement of manufactures, as well as the sciences, will you favour me with a place for the following queries, on

on a buſineſs but little known? And probably, amongſt your ſubſcribers, they may be anſwered through the ſame medium.—How, and at what time, was hat-making invented, and by whom?—Were there ever any engines made uſe of in that buſineſs?—Are there any at preſent? And if ſo, in what part of the country are they made uſe of? And how far do they go in the proceſs?—Are there any machines for the cutting of hare, rabbit, or beaver ſkins? And if any, where are they worked?—Your inſerting the above in your next number will oblige, Sir, yours,

N. L.

There is no mention made of the above, either in the Tranſactions of the Royal Society of London, or in Beckman's Hiſtory of Inventions.

THE above did not come to hand early enough for me to make any enquiries reſpecting the invention, hiſtory, and practice of the art of hat-making. Reſearches of the kind pointed out by this correſpondent are peculiarly adapted to the views and intention of a Journal of the Arts; and I hope he will not be diſappointed in his expectations from others of my readers, whoſe purſuits may have enabled them to throw light upon the ſubject. At all events, I ſhall certainly have ſome information to communicate in the next number, which, for want of time, I cannot at preſent arrange and digeſt.

W. N.

X.

New Obſervations on the Method of producing very loud Fulminations with various Bodies, by Means of Phoſphorus. By Cit.* BRUGNATELLI.

I WAS aware that the oxygenated muriate of potaſh produces effects ſuperior to nitre, when mixed with charcoal and ſulphur and converted into gunpowder; that it detonates by percuſſion or trituration with a great number of combuſtible ſubſtances; but I did not expect to produce effects much more conſiderable by uſing the ſimple nitrates, and even the metallic oxides mixed with phoſphorus and ſtruck with a hammer.

Experiment 1. I took a gros of the cryſtallized nitrate of ſilver, which I placed on an heavy anvil, and laid a very thin ſlice of phoſphorus in the midſt of the cryſtals. The temperature of the atmoſphere during theſe experiments was not higher than 6° above the zero of Reaumur's thermometer. The materials being thus diſpoſed, I ſtruck the mixture rather ſmartly with a hammer. The conſequence was one of the moſt terrible detonations I ever witneſſed, which ſhook the anvil and its wooden ſupport. Streaks of ſilver were obſerved upon the anvil five or ſix lines in length. One of the edges of the hammer was bended and turned upwards. I was perfectly ſtunned, and my clothes were torn in various places.

* Tranſlated by Van Mons from the Italian manuſcript of the author into French, and inſerted in the Annales de Chimie, xxvii. 72.; from which work the preſent tranſlation is made.

I have

I have frequently repeated the ſame experiment, even in my lectures; and though I operated with no more than a few grains of the ſalt, the noiſe of the exploſion was never leſs than that of a muſquet.

The lapis infernalis has nearly the ſame effect as the nitrate of ſilver.

Experiment 2. I placed on an anvil a pinch of the oxygenated muriate of potaſh with a ſmall portion of phoſphorus, and ſtruck the mixture with a hammer. The detonation was exceſſively ſtrong.

Experiment 3. The dry nitrate of biſmuth detonated very ſtrongly. The ſame effect was produced with all the metallic nitrates which were to be found in the laboratory, and particularly with the nitrate of tin.

Experiment 4. I repeated the ſame experiment with the fuſed nitrate of mercury, of which I put ſix grains with a ſmall quantity of phoſphorus on an anvil, and ſtruck them with a hammer. The phoſphorus ſimply took fire, without producing any noiſe: but when the hammer was ſlightly heated, the ſame mixture detonated with a ſhocking noiſe. After the fulmination the mercury was found reduced, having, as it were, ſilvered the anvil in very brilliant radiations.

Experiment 5. I afterwards tried the alkaline nitrates, particularly that of potaſh. When a ſmall ſlice of phoſphorus was laid on a pinch of nitre and ſtruck with a cold hammer, no fulmination was produced even by repeated blows; but having ſlightly heated the hammer, in order that the affinities might act more deciſively, the very firſt blow produced a very loud fulmination, incomparably ſtronger than that of gunpowder.

Experiment 6. I obtained no fulmination or detonation with the ſulphates of alumine, of copper, or of iron ſtruck with phoſphorus in the before-mentioned manner, though I heated the hammer more than uſual.

Experiment 7. Neither did I obtain any fulmination from the ſimple muriate of ſilver, commonly called luna cornea, by the ſame treatment with phoſphorus.

Experiment 8. I likewiſe tried the oxygenated muriate of ſilver, obtained by decompoſing the nitrate of ſilver by the oxygenated muriatic acid. A pinch of this ſalt (with phoſphorus), ſtruck with a warmed hammer, produced a much weaker fulmination than was obtained with the cryſtallized or fuſed nitrate of ſilver, or the other ſalts before mentioned. The oxygenated muriate of mercury, treated in the ſame manner, afforded a very weak detonation.

Experiments 9 *and* 10. I endeavoured to produce fulminations with the metallic oxydes treated with phoſphorus. Thoſe of manganeſe, of zinc, of copper, of iron, of antimony, and of lead, produced no effect, even when ſtruck with the hammer conſiderably heated; but I obtained fulminations with the yellow oxyde of mercury (turbith mineral) and the grey oxyde of the ſame metal.

Turbith mineral does not fulminate in contact with phoſphorus when it is pulveriſed. It muſt be in a lump to produce this effect. The ſame thing happens with regard to the grey oxyde of mercury by the nitric acid, except that the fulmination is ſtronger.

Experiments 11 *and* 12. I was curious to aſcertain whether I ſhould obtain ſimilar fulminations with the ſalts before mentioned, by ſubſtituting another acidifiable combuſtible ſubſtance inſtead of phoſphorus; as, for example, ſulphur and charcoal.

I accordingly took nine grains of lapis infernalis and three grains of pulveriſed ſulphur, which

which I ftruck with the cold hammer. The fulphur took fire without affording any noife; but, when the hammer was ufed hot, a detonation was heard, and rays of filver appeared on the anvil.

Having repeated the fame experiment with lapis infernalis and charcoal, I could produce no more than a very dull detonation, though the hammer was well heated.

Experiment 13. I took feveral of the falts which had fulminated by the ftroke, and threw them, together with the phofphorus, into the liquid oxygenated muriatic acid; but no detonation was produced. In an atmofphere of the gas of the fame acid, the fulminating mixtures burned with a flight crepitation, fimilar to that which is excited by phofphorus alone.

Experiment 14. I wrapped moft of thefe detonated mixtures in fmall pieces of paper, and threw them one after the other into a red-hot crucible. They burned with a very lively flame, but did not fulminate nor detonate.

Conclusion.

1ft. THE nitrate of filver, whether cryftallized or fufed, fulminates when ftruck with a hammer, together with phofphorus, even at a low temperature. *Exp.* 1.—All the falts of filver do not fulminate equally, nor in the fame manner. *Exp.* 7.

2d. Moft of the metallic nitrates fulminate with phofphorus. *Exp.* 3 and 4.

3d. Common nitre, of which the fulminating property was before known, in its mixture with various combuftibles, elevated to a certain temperature, or put into contact with an inflamed body, as in gunpowder or fulminating powder, is now found to detonate with phofphorus alone. *Exp.* 5.

4th. Thofe falts into which the nitric acid has not entered as a component part do not fulminate.

5th. The oxygenated muriates of potafh, of filver, and of mercury, fulminate with phofphorus, but the latter with much lefs effect than many other falts. *Exp.* 8 and 2.

6th. The falts are not the only bodies which fulminate with phofphorus: feveral metallic oxydes have the fame property. *Exp.* 9 and 10.

7th. Phofphorus likewife is not the only acidifiable and folid combuftible matter capable of producing fulminations. Charcoal produces the fame effect at a more elevated temperature. *Exp.* 11 and 12.

8th. Thofe fubftances which fulminate with phofphorus produce no effect when thrown into the liquid oxygenated muriatic acid. *Exp.* 13.—Neither do they fulminate when expofed together with phofphorus to an elevated temperature. *Exp.* 14.—The blow of the hammer is neceffary to throw the component parts of thefe bodies into a ftate of ofcillation, in order to determine the affinities with effect.

ADDITION BY VAN MONS.

THE facts obferved by the learned editor of the Italian Annals of Chemiftry appeared too curious and important for me to lofe any time in repeating his experiments. My fuccefs was as follows:

The

The cryſtalliſed nitrate of ſilver detonated very ſtrongly, emitting only a weak flame. The ſilver was reduced to the ſtate of a blackiſh oxyde.

Lapis infernalis alſo detonated, but much more weakly. The metal was completely reduced.

The nitrate of tin fulminated with conſiderable ſtrength.

The fuſed nitrate of mercury produced a ſtrong effect.

The different oxydes tried by Brugnatelli, that of manganeſe excepted, produced no noiſe by friction; and the detonations under the ſtroke were at leaſt doubtful. The oxydes were all in part reduced, and the mixtures burned in a lively manner, throwing out ignited ſparks to a diſtance; but the phoſphorus alone, after three or four ſtrokes with the hammer, exhibited the ſame appearance.

The oxygenated muriate of mercury afforded perceptible marks of detonation.

I produced theſe ſeveral fulminations without heating the hammer for any one of them. Friction, or a few ſlight previous blows, heat the materials ſufficiently to cauſe the effect to take place with the laſt ſmart blow.

I tried a great number of times to make the experiment with ſulphur and lapis infernalis. In ſome inſtances I rubbed the two ſubſtances together with all my force in iron or marble mortars, and in others I ſtruck them with a heated hammer; but I did not obtain the moſt feeble detonation. The matter itſelf did not even take fire, excepting when I ſtruck it ſmartly in an iron mortar ſtrongly heated with a peſtle equally hot. I was not more fortunate with charcoal. Similar experiments, which I have ſince tried with various other ſubſtances, afforded me the following reſults:

The nitrate of gold by evaporation produced a louder detonation than is afforded by the oxygenated muriates of alkali. The marble mortar in which I made the experiment was covered with ſmall particles of exceſſively thin plates of reduced gold. The metal ſeemed to have undergone a very liquid fuſion.

The muriate of mercury detonated with a force at leaſt equal to that of the nitrate of gold. The metal was almoſt entirely diſſipated.

The nitrate of lead produced a weaker detonation than the foregoing. The metal was found reduced to the ſtate of black oxyde.

The oxygenated muriates of antimony, zinc, and tin, produced a much weaker effect. I ſucceeded, however, once in producing a very ſtrong and inſtantaneous detonation with the firſt of theſe ſalts.

The oxydes of gold, ſilver, and mercury, by fire, were found to occupy the firſt rank among the fulminating ſubſtances. The oxygenated muriate of potaſh does not afford effects equally conſtant with thoſe of the oxydes.

The oxyde of lead, in its different degrees of oxydation, afforded in no inſtance any appearance but that of inflammation.

I formerly exhibited in my public lectures the detonation which may be produced by percuſſion in a mixture of the nitrate of potaſh and phoſphorus. I had even produced exploſions ſufficiently violent to induce the audience to ſuppoſe that I operated with the oxygenated muriate. I ſubmitted the nitrate of ammoniac to the ſame treatment, and produced a detonation ſo terrible that it ſhook the doors of my laboratory.

 The

The nitrates of barytes, of ſtrontian, and of magneſia, by deſiccation, detonated with nearly the ſame force as the nitrate of potaſh.

I had ſtill remaining five grains of the oxygenated muriate of ammoniac, of which I took the half, with about four grains of phoſphorus. I ſlightly cruſhed theſe two ſubſtances, with the intention of mixing them together, when a detonation ſo terrible was heard that the whole houſe was alarmed. The phoſphorus was for the moſt part thrown upon my hat, which it burned for a long time before I perceived it. The violence of the blow cauſed the hammer to fly out of my hands. I very diſtinctly felt it raiſed by the expanſion of the gaſes. The other half of the oxygenated muriate of ammoniac was placed alone upon the bottom of an inverted iron mortar, and ſtruck rather briſkly with the cold hammer. The firſt blow cauſed it to detonate, but no light was diſengaged. I regretted that no more of this ſalt remained to make the experiment with ſulphur and charcoal.

I alſo ſtruck the nitrate of ammoniac alone. The fourth blow cauſed it to fulminate. I ſay it fulminated, becauſe the detonation was accompanied with a diſengagement of light.

I obtained an effect equally conſiderable from the white nitrate of mercury and ammoniac. I think that all the nitro-ammoniacal triple ſalts will detonate more or leſs ſtrongly with phoſphorus.

I was deſirous of trying the ſolid oxygenated muriatic acid; for which purpoſe I cauſed this acid to cryſtallize by artificial cold. A few cryſtals of this acid were put together, with a ſmall piece of phoſphorus, upon an iron mortar placed in a cooling mixture. When theſe were ſtruck with a hammer, a dull detonation was heard at the ſixth or ſeventh blow. The fuſed acid was projected to a diſtance.

I repeated a great number of theſe experiments with pyrophori and the phoſphori called artificial carefully prepared; and in almoſt every caſe I obtained a detonation. I ſhall deſcribe my experiments on theſe ſingular inflammable preparations in a ſeparate article.

Whenever I ſtruck the before-mentioned mixtures with a heated hammer, I always obtained a weaker detonation, at the ſame time that the inflammation was ſtronger; and when the hammer was too hot, no detonation or noiſe was produced. This obſervation I had already made with the oxygenated muriate of potaſh; and it appears to me to explain the phenomena of detonation. The blow with the cold apparatus, by ſtrongly compreſſing the matter, and perhaps exciting ſome heat, produces a partial combuſtion of the phoſphorus, and conſequently engages only part of the oxygen of which the other portion aſſumes the elaſtic ſtate, and produces the noiſe of the detonation. At a very elevated temperature the effects are not altogether the ſame. All the oxygen is at once employed to burn the combuſtible, whence the inflammation is the ſtrongeſt, and no detonation takes place. In fact it is obſervable, that the more ſudden and ſonorous the noiſe, the leſs is the developement of fire; and the contrary. To this principle alſo is referable the obſervation made by Brugnatelli, which I have found to be true, that the oxygenating body muſt in ſome inſtances poſſeſs the form of a lump to obtain the effect; becauſe it applies too extenſively to the combuſtible matter when powdered.

Theſe experiments ſucceed much better by friction than by a blow, and the effects take place more readily upon a rough than a ſmooth body. I uſually place the two ſubſtances on the bottom of an inverted marble mortar, which is left rough from the ſaw.

Upon

Upon these I press the face of an iron hammer, holding the handle with the left hand, and suddenly slide the hammer forward so as to produce a kind of shock. In this manner the detonation or noise is likewise more distinct, and its force more easily determined. Simple compression produces the detonation in many cases.

I think it proper to warn those who are not familiar with experiments of this kind, that when the operation is performed with more than a grain and a half, or at most two grains of phosphorus, there will be danger of burning their hands or clothes by the excess of this combustible, which flies off in a burning state.

I cannot finish this article without restoring to its true proprietor the portion of honour which is due to the discoverer of the detonating property of the oxygenated muriate by percussion or strong friction, which Citizen Fourcroy has attributed to me in the Memoir * he has communicated to the National Institute, no doubt in consequence of my having mentioned this phenomenon in my edition of the Philosophy of Chemistry, under the article of *Oxygenated Muriates*. Professor Wurzer, of Bonn, was the first who observed this fact, by triturating with some force, in a mortar, a mixture, scarcely weighing a grain and a half, of three parts of the oxygenated muriate of soda and one part of sulphur. He obtained a detonation which rendered him deaf for several days. See Crell's Chemical Annals for the year 1792, vol. ii. page 402.

TUNNEL BENEATH THE THAMES.

Reports, with Plans, Sections, &c. of the proposed Dry Tunnel or Passage from Gravesend in Kent to Tilbury in Essex, demonstrating its Practicability, and great Importance to the two Counties and to the Nation at large; also, on a Canal from near Gravesend to Stroud, with some Miscellaneous and Practical Observations. By R. Dodd, Engineer. Quarto. 28 pages, with 3 plates: viz. 1. Plan and Sections of the proposed Tunnel. 2. A View of Gravesend and Tilbury, with the Section of the River, shewing the Strata and Depth of Water: and, 3. A Map of the Country within twenty Miles of Gravesend. London, printed for J. Taylor, 1798.

SOME account of this undertaking was given in this Journal a few months ago. (II. 239.) The present Reports will afford satisfactory information of its detail and progress, and the public will hear with pleasure that it is likely to be carried into effect.

In the month of May 1798, Mr. Dodd circulated an Introductory Report or Address to the Nobility, Gentry, &c. of Essex and Kent, in which he states that the extended scale of commerce on the river Thames forbids the construction of a bridge at Gravesend; but that the practice of making passages, tunnels, or drifts under rivers has been adopted to a great extent in various parts of the kingdom; namely, at the coal works under the rivers Tyne and Wear, and at Whitehaven under the very ocean; that a tunnel is actually intended to be made under the mouth of the river Tyne, to answer the purposes of a bridge for carriages and passengers;—that the measure now proposed will save a circuit of near fifty

* Philosophical Journal, I. 168.

fifty miles to carriages passing from one county to the other; and that the situation at or near Gravesend is peculiarly eligible from its natural facilities, as the greatest part of the ground to be passed through is chalk. These observations are followed by the proposal and estimate which have already been given at the page of our Journal last quoted. Mr. Dodd observes that 12 l. per running yard must be considered as a handsome allowance, since it is a fact, that some of the most considerable tunnels in this kingdom on a scale of nineteen feet by seventeen, which is larger than the tunnel at present recommended, have been executed at that price.

On the 18th of July 1798, a meeting was held in the Town Hall of Gravesend, the Earl of Darnley in the chair; when it was resolved, That there being no reason to doubt the practicability of so desirable a measure, the co-operation of government, the assent of the neighbouring land-owners, or the willingness of the public to subscribe to its accomplishment, the opinion of the meeting was, that transferable shares of 100 l. each would afford the best means of effecting it; and that a subscription should be opened at the office of Messrs. Evans and Son, at Gravesend, for its support and encouragement.

In the report and estimate which the engineer presented to this meeting, he states, that from the actual survey he finds no reason to add to the former estimate, but rather to abate somewhat more than 3000 l. on account of the excellent chalk which he finds will be afforded at the most eligible place for the excavation. The deviation from an horizontal line of the tunnel, which passes at about thirty feet below the bed of the river, will be four inches in the yard. Several military and commercial advantages of the undertaking are likewise clearly stated, and an estimate given of the cost of a twelve feet tunnel, admitting carriages to pass only one way at a time, in case the sixteen feet tunnel should be thought too expensive. In the subsequent observations the author enlarges still more upon the same interesting topics, and makes a comparison between the communication by tunnels and the great modern improvement of iron bridges, which last erection he shews to be much less durable, and more liable to speedy destruction by an enemy, than the tunnel. He remarks, that the destructive elastic fluids, which are known to be extricated in mines, cannot affect the present work when once finished, because of the clear communication with the external air at both ends; and he gives some practical remarks on the discovery of inflammable air or fire-damp, which, on account of the interesting nature of the subject, I shall here transcribe.

"On entering any excavation under ground, where this fire-damp is suspected to lie, the candle ought to be held in the left hand (the smaller the candle the more to be depended upon), and the flame thereof to be shaded by the right-hand fingers being placed horizontal, and on a level with the top of the flame of the candle. If the air be good, and perfectly free from any inflammable air or hydrogen, a small brown top or pinnacle is seen on the top of the flame, the same as is seen in the chamber of a house; but on approaching the real fire-damp another top or spire is seen of a blue colour, above the brown just described as arising therefrom; and on advancing a step or two forward the damp is commonly more dense, and consequently the appearances over the candle assume a more serious aspect by the brown spire or small top beginning to disappear; another step perhaps reduces it altogether, on which the danger commences; and the spire or top sits down altogether blue on the flame. At this last stage it is not safe to proceed further without great

great circumſpection joined with ſteadineſs: a little further the blue top is ſeen to be more perfect and denſe, which if general through the place, all lights ſhould be extinguiſhed, and the workmen be made to quit the place. Perhaps on advancing a very little further a light blue bead appears to circumſcribe the other dark blue, as if approaching to flame, and having a more than ordinary pointed ſpire terminating like a white thread; and the danger becomes very great if to remain any time in this poſition. The next ſtage ſhews the ſprents to have taken place, which ſprents are ſimilar to thoſe produced by ſqueezing the oil from the rind of a lemon into the flame of a candle: at this period ſometimes has been obſerved a ſmall buſhy dark-coloured cloud hanging over the top of the flame, ſtill more and more attracting the fire; when in an inſtant perhaps after this laſt obſervation the whole appearance expands into flames, and one general conflagration takes place. The miſchief which enſues is in proportion to the accumulated quantity of this pernicious vapour; which if great flies to the neareſt aperture leading to the atmoſphere, being about eleven times lighter than common air, with a dreadful exploſion and extraordinary eruption. But in caſe the ſaid fire-damp or inflammable air be mixed with ſtyth, or black damp, theſe appearances are ſomewhat varied; and by being mixed with a very dark brown is in general more ſafe.

"Black damp or ſtyth" (doubtleſs carbonic acid, or hydro-carbonate) "which ariſes in mines is directly the reverſe of inflammable air or fire-damp; the former extinguiſhes the flame of a candle as quickly as if put into water or any other fluid; the other cauſes the candle to burn too faſt. This ſtyth or black damp prevails moſt where there is little or no inflammable air, and when the due circulation of atmoſpheric air is neglected."

It was of eſſential conſequence, by way of ſhewing the practicability of the preſent undertaking, that Mr. Dodd ſhould impreſs the minds of his employers with the fact that ſuch tunnels had been made with ſucceſs on a much larger ſcale. For this purpoſe he relates various hiſtorical matters, beſides thoſe already mentioned, from which we may notice, that the earlieſt tunnel for inland navigation was executed by M. Riquet, to convey the canal of Languedoc through a mountain near Bezieres. The firſt executed in this country was by the celebrated Brindley on the duke of Bridgewater's canal near Mancheſter. The next is the famous tunnel of Hare Caſtle Hill, in Staffordſhire, by the ſame engineer, which is 2880 yards long, and paſſes through a variety of ſtrata, quickſands, &c. more than ſeventy yards below the ſurface of the earth, and ſerves as the receptacle for part of the Grand Trunk Canal. The tunnel of Saperton is two miles and three quarters long, and was carried through two miles of ſolid rock. Many other drifts or tunnels have been made in this kingdom, through rocks and obſtacles of various kinds, in a courſe of time and at expence much leſs than would be apprehended by perſons unacquainted with works of this nature.

Mr. Dodd's report and eſtimate on the projected dry tunnel under the river Tyne contains illuſtrations and argument of nearly the ſame import. His pamphlet concludes with a propoſal for an inland canal from the Thames near Graveſend, to the Medway near Stroud; which, by a ſhort courſe of about ſix miles, would ſave the circuitous and leſs certain paſſage of forty-ſeven miles by the way of the Nore. The peculiar advantages of this cut to the country at large, and to the government eſtabliſhments on theſe rivers, as well as the nature of the ground itſelf, are ſtrongly and perſpicuouſly ſtated.

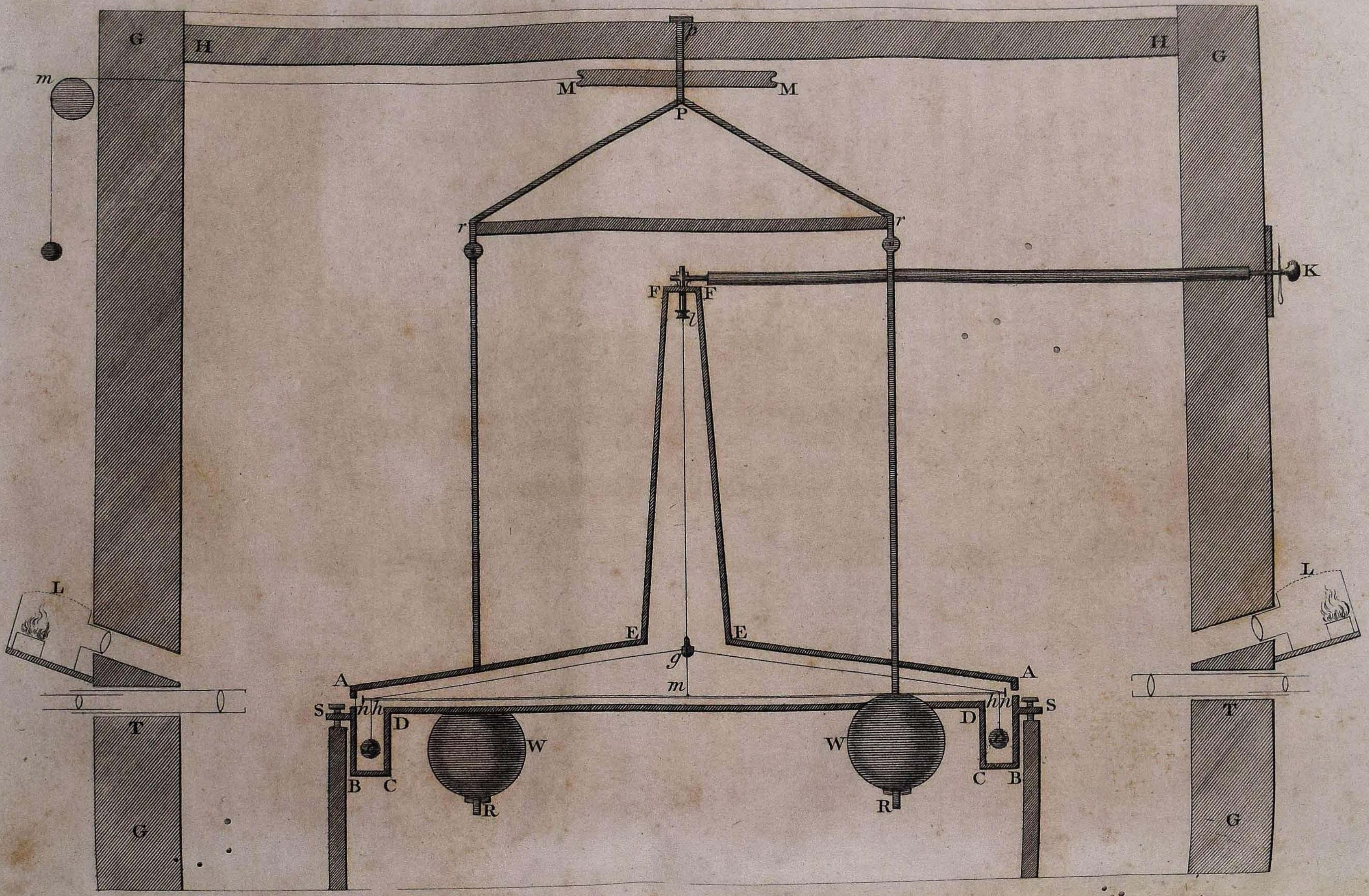
The

The last public meeting on the former business was held at the London Tavern on the 14th of last month (December); when it appeared by the Report that the subscriptions amounted to upwards of ten thousand pounds. The managing committee of subscribers consists of the Earl of Darnley, Lord Petre, the Hon. J. T. Townshend, M. P. the Hon. Robert Petre, Sir William Geary, Bart. M. P., Mr. Ald. Lushington, M. P., John J. Angerstein, and Claude Scott, Esqrs. Lieut. Col. Twiss, Royal Engineers, Capt. Schank, R. N. T. Woodruffe Smith, Esq., John Mavor, Esq., Geo. Hawks, Esq., and Benjamin Harrison, Esq.—Claude Scott, Esq. is Treasurer.

The undertaking will be begun and carried on by those who shall first subscribe to the proposed shares, which are three hundred at a hundred pounds each; but the actual work will not be commenced till the whole sum of thirty thousand pounds shall have been subscribed.

A fellow-labourer in the cause of science, Mr. A. Tilloch, objects totally to the account I gave of Mr. Cartwright's apparatus for rendering the pistons of steam-engines tight by metallic fittings, at page 365 of the present volume. As the whole of his remarks appear to me to be obviously erroneous, and the matter is before the public, I must be excused from entering into controversy. He will himself perhaps, on second thoughts, recollect that a circle, even though in diameter equal to that of the earth, will not be converted into a triangle by cutting it in three pieces; and that it is absurd to suppose the third law of nature, that *action and re-action are equal and contrary*, can either be dispensed with or explained away. He will then probably look (not to the vacuum, but) to the lower plate of Mr. Cartwright's piston for the re-action; which, by means of the piston rod and the work required to be done, is made to act beneath the moveable pieces of the apparatus, while the steam presses their upper surface with no inconsiderable force; admitting its elasticity to be equivalent to a single atmosphere only: a force sufficient, in my apprehension, to prevent ground surfaces from sliding freely, if at all, upon each other. But, as we are all liable to mistakes in new practical matters, I could have wished that Mr. Tilloch had reasoned less diffusely, and referred at once to the facts. If it be true that there was a steam-engine at work six months ago, when Mr. T. invited the public to inspect it, at Mr. Rowley's, in Cleveland-street, Marylebone *; or if he can bring evidence that such an engine has been at work for any considerable part of the time since he published his description, and that the facility of operation, the power, and the durability of the apparatus are such as he conceives them to be;—I cannot but think that he has, unfortunately, overlooked his best argument. To this argument, if offered, I must grant my assent; and I beg leave to assure him that I shall most readily attend, inspect, and report concerning this engine, if he or the inventor will give me the opportunity, without in the least regarding whether this part of my duty to the public shall confirm or overthrow those opinions which the present state of the facts has compelled me to give.

* I went there at the time, and saw the parts of a small model or engine, which was not at work; and, upon late enquiry, I understand there is none there at present. I could gain no information whether there was any engine of this construction at work, or in progress, at any other place.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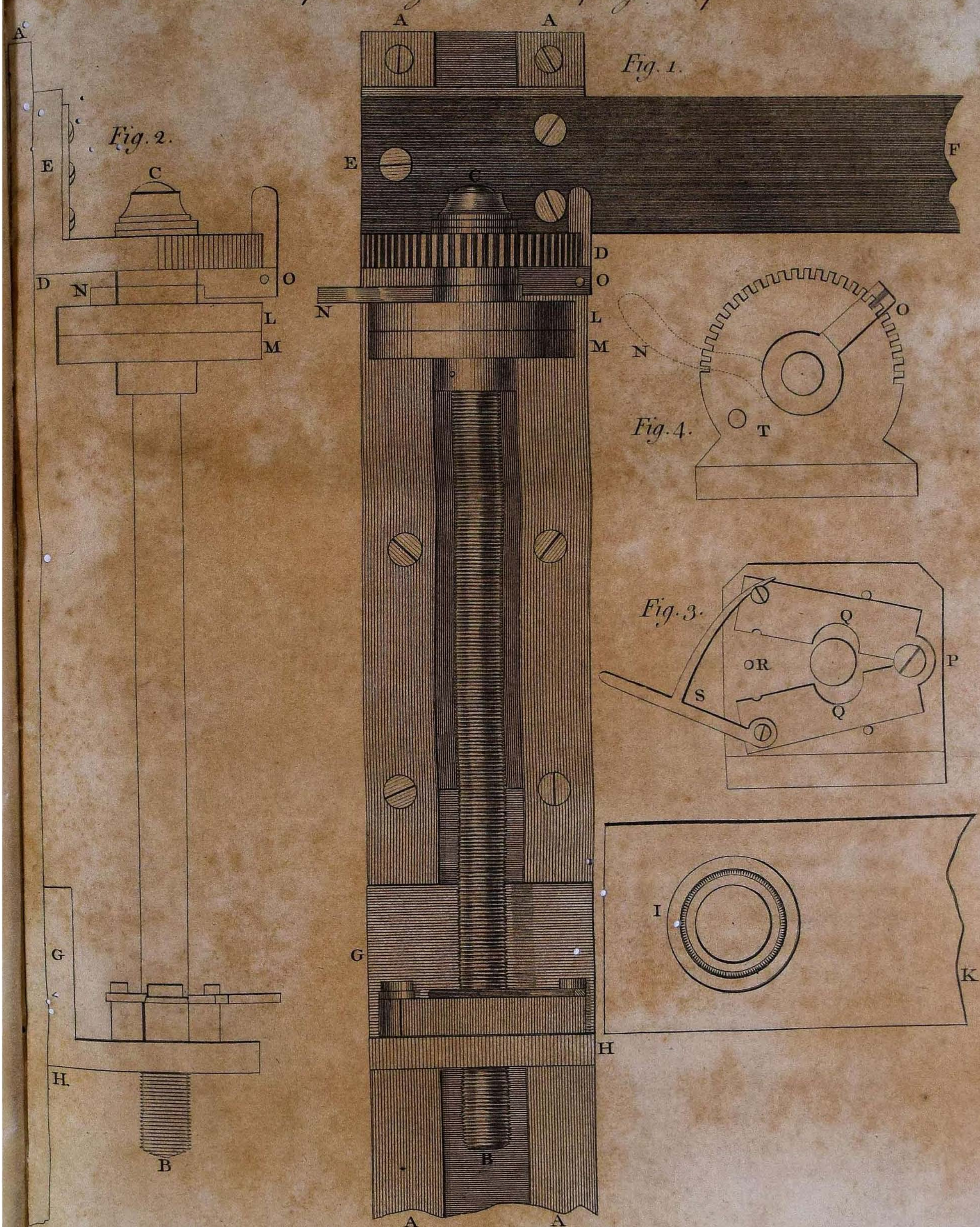

Barlow sculp.